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THE

EDINBURGH NEW
PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE
PROGRESSIVE DISCOVERIES AND IMPROVEMENTS
IN THE
SCIENCES AND THE ARTS.

CONDUCTED BY

ROBERT JAMESON,

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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

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THE late Dr Henry was born in Manchester, on the 12th of December 1774. His earlier years were passed amid influences propitious to the nurture of those tastes for which he was afterwards distinguished. His father, Mr Thomas Henry, F.R.S., formerly president of this society, was a zealous cultivator of chemical science, to which he devoted all the leisure moments of a life actively engaged in medical practice, with a perseverance and success that have been affectionately commemorated by his son.[†] The earliest impressions of Dr Henry's childhood were, therefore, such as to inspire interest and reverence for the pursuits of science; and he is said, when very young, to have sought amusement in attempting to imitate, with such means as were at his disposal, the chemical experiments which his father had been performing. A severe accident, which befel him in early life, by disqualifying him for the active sports of boyhood, must also have contributed to determine his taste for books and sedentary occupations. This injury, occasioned by the fall of a heavy beam upon his right side, was so serious as

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[*] Read before the Literary and Philosophical Society of Manchester.

[†] Manchester Memoirs. 2d Series, vol. iii.

at the time to endanger life and materially to check his growth, and left as its consequence acute neuralgic pains, which recurred often after long intervals of remission, and with peculiar severity some months before his death. His fortitude, while yet a child, in supporting the sudden paroxysms of pain, which were often so intense as to oblige him to rest in the streets, was most remarkable;—and, in his efforts to banish the perception of physical suffering by an absorbing mental occupation, he already manifested that energy of resolution and purpose, which throughout life compelled a feeble bodily frame to keep pace with the exertions of an ardent and unfatigued spirit.

Dr Henry's earliest instructor was the Rev. Ralph Harrison, whose repute, as a teacher of the ancient languages, was so widely spread, as to draw to Manchester the sons of persons of rank from a distance, and, among others, those of the Marquis of Waterford, attended by their accomplished tutor Mr de Polier. On the establishment of an Academy in Manchester, which has since been removed to York, Mr Harrison was chosen to fill the chair of classical literature. His pupil had made such rapid progress as to be permitted, though considerably under the customary age for admission, to follow his preceptor to this enlarged sphere of competition. Here, though struggling with older and more advanced classmates, his diligence and ardour were rewarded by the approbation of his academic superiors, and he received in the prize allotted to him—an elegant copy of Virgil—the earliest of those literary distinctions, which, throughout life, constituted the main objects of his ambition.

Immediately after leaving the academy, Dr Henry had the good fortune to succeed Dr Holme as an inmate in the house of that accomplished scholar and enlightened physician the late Dr Percival. A constant liability to violent headachs, combined with weak eyesight, prevented Dr Percival from writing or reading with the vigour and continuity essential to his various literary pursuits. It was the duty, therefore, of Dr Henry and of other young persons, who occupied the same place before or after him, to read aloud to Dr Percival, and to conduct, after his dictation, the extensive correspondence which he maintained with those most eminent in science and in letters. Dr Percival's style was peculiarly correct and elegant; and his example and judicious counsels seem to have been most instrumental in guiding the tastes of his young companion, and in establishing habits of vigilant and appropriate expression. Dr Percival also directed, with great judgment and kindness, his course of reading, and particularly recommended to him works on mental and ethical philosophy;—thus probably laying the foundation of that taste for enlarged speculation on the moral and intellectual nature of man, and of that faculty of delicately discriminating the finer shades of character and of genius, which contributed so largely to his sources of enjoyment and of fame. Of the salutary influence upon his character, of such intimate communion with this learned and high-minded physician, he was accustomed often to speak in after-life with grateful remembrance; and was ever ready to pay his warm testimony to the varied and tasteful scholarship, the enlarged philosophy, and the pure and elevated moral bearing of his distinguished instructor. In a letter, many years ago, addressed to the writer, Dr Henry speaks of Dr Percival as "an illustrious pattern of every thing delicate and pure in sentiment, elegant and dignified in taste, and polished in address and manner;—a man who, while he would have adorned a court by his gentlemanly demeanour, yet paid a tender and unceasing attention to the feelings of the humblest of those by whom he was habitually surrounded."

In this improving residence, Dr Henry remained during five years, which were devoted to the general culture of his mind, and to the preliminary studies of his profession. About the close of this period he first engaged in the practical observation of disease in the Manchester Infirmary, where he enjoyed the instructions of another of those eminent physicians, who have conferred so much literary glory on this town and on this society, the late Dr Ferriar. In his invaluable "Medical Histories," the systematized records of his experience in our great public charities, Dr Ferriar has left to the profession the finest existing models of what such narratives of disease ought to be —in style, simple, concise, and energetic, though not rejecting, on suitable occasions (as in his moving essay on the Treatment of the Dying), the warmer colouring suggested by deep feeling;—in substance, deriving general pathological conclusions, from the accumulated and methodized results of an experience no less ample than sagaciously directed and interpreted. As a hospital physician, Dr Ferriar seems to have possessed, in an eminent degree, the faculty of at once eliciting truth from the obtuseness or reluctance of the suffering poor, by his abrupt and pointed interrogatories, and by his impatience of all irrelevant matter. He was especially distinguished by strength and rectitude of understanding, by manners perhaps somewhat unbending and severe, by a high sense of honour, and by a fearless and dignified moral bearing. To his pupil, his manner was always friendly and encouraging;—and out of this early intercourse issued the sources of mutual esteem and permanent friendship, which were strongly evinced by his confiding, during illness, his medical duties to Dr Henry, and also by his making choice of him as his own attendant in the successive seizures which preceded his death.

After having been thus initiated in those pursuits to which his after life was to be mainly dedicated, Dr Henry was removed in the winter of 1795–6 to the University of Edinburgh, at that time in its highest repute as a school of medicine and of the natural sciences. The chair of chemistry was still occupied by the venerable Dr Black, whose discovery of the facts that establish the existence of heat in a latent form, and whose successful discrimination between the caustic earths and their carbonates, had raised him to the highest rank among chemical philosophers. Dr Henry was an eager hearer of the beautiful prelections in which Dr Black, with calm and simple dignity, unfolded, in exact and perspicuous order, the truths of a science, which may almost be said to have been first called into existence by himself and his contemporaries. Dr Henry's early kindled love for science was strengthened by lessons so impressively taught, in which, reverence for the teacher was interwoven with intense delight in the subject matter of his instructions, and, especially, with a glowing admiration of that successful inductive process which had guided to the discovery of latent caloric. Dr Henry was no less fortunate in his other instructors, both in general and professional knowledge. The important chair of Practical Medicine was then filled by Dr

Gregory, whose marked originality of thought and humour, and whose happy talent of arresting attention by illustrative cases, narrated with dramatic effect, threw around the descriptions of disease a fascinating interest, to which they would seem naturally most alien. On Dr Henry's second visit in 1805 to the University, he found the chair of Physical Science adorned by the profound mathematical learning of Playfair, and that of Moral Philosophy occupied by Stewart, whose pre-eminence as a teacher has been beautifully celebrated by the most competent judge of modern times.* Of the invaluable instructions of Mr Stewart, Dr Henry was prevented from availing himself by the necessity of following at the same hour some professional lectures; but he has confessed that he not unfrequently deserted the Clinical Theatre for the impressive lessons of a higher wisdom. He ever retained and expressed the deepest admiration for the compositions of these two master minds. The style of Playfair, in his Dissertation on Physical Science, and in his biographical notices of Hutton and Robison, Dr Henry regarded as, upon the whole, the best adapted to philosophical purposes which our language possesses, and he had certainly erected it into a standard for his own imitation. In the writings of Stewart he was accustomed to praise the delicacy and correctness of his taste in arts and letters, the easy and melodious flow of his periods, his graceful distribution of ornament; but above all, the elevation and purity of his moral judgments and sensibilities, and the fervour and depth of conviction, with which he ever advocates as inseparable, the interests of philosophy and of virtue.

To this period of his life, Dr Henry always looked back, as a season of pure and unmingled happiness, arising out of the consciousness of a steady and rapid progress in knowledge, undisturbed by the cares and practical business of life, and quickened by constant intercommunion with minds ardently devoted to similar pursuits. He seems, indeed, to have been peculiarly happy in his intimate associates and fellow-labourers. Nor has the Metropolis of the North ever before or since assembled within its halls of science, either so many illustrious teachers, or so noble a company of hearers. Among the latter were numbered Marcet, Roget, De la Rive, Thomson, Allen, Scarlett, and Jeffrey; and Lord Brougham as a youthful disputant in the Speculative, and occasionally in the Physical and Medical Societies, was giving evidence of those native energies of genius and character, which have since borne him to the highest judicial and political station in this country.

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* Sir James Mackintosh, Preliminary Dissertation, p. 386.

Breathing such an exciting atmosphere, and urged onwards by his own ardent spirit, Dr Henry devoted his entire time and strength to mental improvement; and has often affirmed that the rest of his life, active as it was, appeared a state of inglorious repose, when contrasted with this season of unremitted effort. It was, therefore, with deep regret, that after a year thus spent, he quitted the University, in deference to prudential considerations, with little prospect of ever revisiting it as a student; and engaged in association with his father in general medical practice. After a few years' experience, however, the inadequacy of his delicate frame to bear up against the fatigues of this branch of the profession became evident, and he was permitted, in the year 1805, to return to the University, where he received, in 1807, the diploma of "Doctor in Medicine."

The period intervening between his two academic residences, though passed in the engrossing occupations of his profession, to which was added the superintendence of a chemical business, many years before established by his father, was yet marked by several important contributions to science. In 1797, he communicated to the Royal Society an experimental memoir, the design of which was to re-establish,—in opposition to conclusions drawn by Dr Austin, and sanctioned by the approval of Dr Beddoes and other eminent chemists,—the title of carbon to be ranked among elementary bodies. His proofs were derived from the electrization of an impure carburetted hydrogen gas; but it is needless to particularize the experiments, since in a subsequent paper he made known a source of fallacy which had vitiated their results. They have value, therefore, chiefly as suggesting encouragement to the young, by shewing that Dr Henry had to pass through a stage of tentative initiation before reaching the consummate skill which afterwards distinguished him in that most delicate province of chemical research.

In the same interval (1800) he also made public, through the Philosophical Transactions, some experiments on the muriatic acid gas. This memoir is one of peculiar interest, both as recalling the state of chemical doctrine, anterior to the grand discoveries of Davy, and as marking the influence of pre-conceived theories on the interpretation of facts. Oxygen was then regarded as the sole principle of acidity, and the muriatic acid was consequently supposed to be constituted of oxygen, associated with an unknown radical. It was in the hope of detaching this imaginary element from oxygen, that Dr Henry exposed the muriatic acid gas, both alone, and mingled with gaseous matter possessing a strong affinity for oxygen, to repeated electrical discharges. When the muriatic acid gas was electrified alone over mercury, its volume was uniformly diminished, hydrogen gas was disengaged, and a white deposit was collected, which proved to be calomel. The decrease of volume and the formation of calomel were much more considerable, when the electric discharges were passed through a mixture of oxygen and muriatic acid gas. When the electrization was performed without the presence of mercury in a glass-tube, closed by stoppers, each perforated with platina wire, chlorine was evolved and detected by the usual test. It is manifest that these experiments, had they been justly interpreted, were sufficient to establish the true view of the composition of muriatic acid gas. Yet, governed by the theory of acidification then universally prevalent, Dr Henry referred the disengagement of hydrogen to the decomposition of water, which was supposed to be still present in the gas after a week's contact with fused chloride of calcium. Nor was it until many years subsequently that the simpler theory was firmly established by the genius of Davy. To the new doctrine, Dr Henry had, however, the merit of becoming an early convert; and in a supplementary essay, published in the Transactions for 1812, he supplied some important evidence in its favour. He shewed that the same proportion of hydrogen gas was obtained by electrizing muriatic acid gas, whether it had been exposed or not to fused chloride of calcium; and hence concluded that the hydrogen was eliminated from the muriatic gas, and not from aqueous vapour. He also ascertained, that the muriatic acid gas, when completely insulated in a closed glass vessel, sustained no change of volume from a succession of electrical discharges. This permanence of bulk was made more apparent by repeating the experiment in a vessel with a neck only one-fifth of an inch in diameter. After admitting water to absorb the undecomposed muriatic acid, there remained one hundred measures of chlorine, and one hundred and forty of hydrogen. In conformity with the law of Gay Lussac, the quantities should have been equal, but the deficiency of chlorine was justly referred to its large absorbability by water. The perfect accordance of muriatic acid gas with the law of volumes was further shewn by the observation that the contraction of volumes in muriatic acid gas electrized over mercury;—a diminution due to the combination of the liberated chlorine with mercury,—is precisely equal to the quantity of hydrogen gas obtained.

In 1803 Dr Henry made known to the Royal Society his elaborate experiments on the quantity of gases absorbed by water at different temperatures, and under different pressures. The absorbabilities of the different gases, under a constant pressure, by water of 55° Fahrenheit, were first accurately measured. Elevation of temperature was found to lessen the amount of absorption, the diminution for each increment of 10° above the standard temperature being equivalent to about $\frac{1}{14}$th of the entire bulk absorbable at 55° Fahr. In investigating the absorption of the same gas under varying pressures, Dr Henry arrived at the simple law, "that water takes up of gas condensed by one, two or more additional atmospheres, a quantity which, ordinarily compressed, would be equal to twice, thrice, &c. the volume absorbed under the common pressure of the atmosphere." This exact proportionality of the quantities absorbed to the pressures, makes strongly in favour of the theory proposed by Dr Dalton, that the absorption of the gases by water is due entirely to mechanical agencies.

Dr Henry described in the Philosophical Transactions for 1808, a form of apparatus, adapted to the combustion of larger quantities of gas than could be fired in eudiometric tubes. Results were thus obtained, which may be pronounced fair approximations to truth, especially when estimated with reference to the still imperfect resources of pneumatic chemistry. The apparatus cannot, however, be now recommended when extreme precision is desirable. In this year (1808) he was elected a Fellow of the Royal Society, and in the following year received, by the award of the President and Council, the medal on Sir Godfrey Copley's donation, as a mark of their approbation of his various papers communicated to the society, and printed in the Philosophical Transactions.

The same analytical method by which the decomposition of muriatic acid gas had been effected, was in 1809 employed by Dr Henry to resolve ammonia into its constituent gases. These experiments may even now be commended as models of extreme accuracy. Ammoniacal gas previously dried with great care, was found to expand to double its primitive volume after a sufficient number of electrical discharges. The average of eight experiments (in five of which the volume was exactly doubled) gave the relation of 100 : 198.78 between the volumes of gas before and after decomposition,—a conformity between theory and experiment, which modern refinement cannot surpass. In this memoir Dr Henry also made known a remarkably elegant and expeditious method of analyzing ammonia, by firing it in a volta tube with a deficient quantity of oxygen. In this process all the ammonia is decomposed, though a part only of the hydrogen thus liberated meets with its equivalent of oxygen. More oxygen is then added to burn the residuary hydrogen :—for Dr Henry had observed, that if the whole quantity of oxygen was added at once, the results were disturbed by the production of nitrate of ammonia. In estimating the proportion of hydrogen and nitrogen constituting ammonia, he obtained somewhat less hydrogen than the theoretical quantity, a deficiency which he rightly ascribed to the cooling agency of so large a volume of azote causing a part of the hydrogen to escape unburned. He afterwards, by employing nitrous oxide instead of oxygen, obtained and published in the memoirs of this society, results that establish precise multiple relations.

The gaseous substances, issuing from the destructive distillation of coal and oil, had very early engaged Dr Henry's attention, and he had at various times devoted much labour both to their chemical analysis and to ascertain their respective fitness for the purposes of illumination. The general conclusions, which he had made known in several successive memoirs, were, that these gaseous products are mixtures of olefiant, carbonic oxide, carburetted hydrogen and hydrogen gases in varying proportions, with other accidental impurities, as carbonic acid and sulphuretted hydrogen. An opposite doctrine had been proposed to the Royal Society by a distinguished chemist. It was maintained that carburetted hydrogen does not constitute a distinct gaseous species, that olefiant gas is the only known compound of carbon and hydrogen; and that the gases obtained from oil and coal are nothing more than mixtures of olefiant with simple hydrogen.

In an elaborate paper published in the Transactions for 1821, Dr Henry succeeded, however, in establishing the soundness of his original views. He contended that the concurring results obtained by Dr Dalton, Sir H. Davy, Dr Thomson, and himself, from the analysis of carburetted hydrogen collected both from stagnant water and from the coal measures, at distant times and places, clearly demonstrate that gas to be a true chemical compound, characterized by perfect uniformity of properties and composition. He proceeded to investigate the action of chlorine both upon carburetted hydrogen and olefiant gases, with a view to learn how far chlorine may be depended upon as a means of effecting their separation. Carburetted hydrogen was found to be wholly unaltered by prolonged contact with chlorine, when light was carefully excluded. Under the same circumstances olefiant gas, on the contrary, was entirely removed by chlorine. Hence he derived a simple and beautiful mode of separating olefiant gas from the other gaseous compounds of carbon with hydrogen or with oxygen, as well as from pure hydrogen. After establishing the perfect accuracy of this process, on artificial mixtures of the gases in known proportions, he applied it to the mixtures of the same gases in unknown proportions, which constitute oil and coal gases. The best oil gas shewed forty per cent. of a gas condensible by chlorine; the best coal gas not more than thirteen per cent. The residuary gases, left after the complete action of chlorine, were then detonated in a volta tube with oxygen, and afforded results, shewing that they were mixtures of carburetted hydrogen, carbonic oxide and hydrogen gases, in proportions which he was then unable to determine except by approximative calculation.—"No one instance," he concludes, "has ever occurred to me of a gas obtained from oil or coal, which, after the action of chlorine upon it, with the exclusion of light, presented a residuum at all approaching to simple hydrogen gas."

In his latest communication to the Royal Society (1824), Dr Henry succeeded in conquering the only remaining difficulty in the analysis of these complex mixtures; viz., the ascertaining, by chemical means, the exact proportions which the gases, left after the action of chlorine on oil and coal gas, bear to each other. For this purpose he skilfully availed himself of the property, which had been recently discovered by Döbereiner in finely divided platina, of determining gaseous combination. Having ascertained that carbonic oxide with oxygen is rapidly converted into carbonic acid, in presence of platina at a temperature of 340° Fahrenheit, while carburetted hydrogen is wholly unchanged, till the heat considerably exceeds that of boiling mercury, Dr Henry exposed a mixture of carbonic oxide, hydrogen, and carburetted hydrogen with oxygen, into which platina had been admitted, to the temperature of 340°. The carbonic oxide and hydrogen were converted into carbonic acid and water,—and their relative proportions easily determined. The residuary carburetted hydrogen was then detonated with oxygen in a volta eudiometer. The numbers thus obtained corresponded precisely with the quantities of the gases that had been mingled together. In further trial of this new mode of operating, it was applied to the analysis of a mixture of the same gases, but in unknown proportions, which constitutes an inferior quality of coal gas. The specific gravity of this gas, derived from the proportions thus ascertained of its constituents, was .303, a number coinciding as nearly as could be expected with the actual specific gravity of the gas .308.

In the course of this enquiry, Dr Henry noticed the curious property of gaseous interference, which was observed about the same time, and first made public by Dr Turner. Though not affecting the main object of his researches, these phenomena attracted his attention by their singularity and novelty; and the experiments he performed in the hope of unfolding their nature, suggested a theory of interference, which has been confirmed by recent investigations.—"The property," he observes, "inherent in certain gases, of retarding the action of the platina sponge, when they are added to an explosive mixture of oxygen and hydrogen, is most remarkable in those which possess the strongest attraction for oxygen; and it is probably to the degree of this attraction, rather than to any agency arising out of their relations to caloric, that we are to ascribe the various powers, which the gases manifest in that respect."

An essay on the compounds of nitrogen, published in the Manchester Memoirs for the same year (1824), though not adding any facts of moment to the prior results of Gay Lussac, yet made known some new and exact methods of speedily decomposing nitrous oxide and nitrous gas. Thus the constitution by volume of nitrous oxide, was determined with singular precision by detonating it with carbonic oxide, instead of with hydrogen gas; and nitrous gas was found to form an explosive mixture with olefiant gas.

It is worthy of remark, that all Dr Henry's scientific memoirs, which have been hitherto enumerated, are devoted to the chemistry of aeriform bodies. For this refined department of science, Dr Henry always manifested the strongest predilection. Indeed of nine experimental papers, contributed by him to the Royal Society, no fewer than eight are dedicated to the gases. At the period when Dr Henry's interest was first awakened for philosophical pursuits, the rapid discovery by Priestley of several new gases, and the sanguine hopes inspired by Beddoes of detecting in these subtle and hitherto concealed forms of matter powerful remedial agents, urged both physiologists and chemists to engage with ardour in pneumatic researches. Subsequent experience has demonstrated, it is true, the unsoundness of these projects for enriching with new resources, the art of practical medicine. But the beautiful law, unfolded by the genius of Gay Lussac, that the gases combine in volumes which are either equal or multiples by an integral number—by establishing, when interwoven with the Daltonian philosophy, the existence of some simple relation between the numbers of atoms existing in equal spaces of aeriform matter, has almost elevated the pneumatic chemistry to the dignity and exactitude of a mathematical science. There is, moreover, in the pursuit of these subtle elements, which escape the visual sense, which can be imprisoned and weighed only by the most refined instruments, and which can be discriminated only by the most delicate processes and indications, a somewhat of mystery and of a higher initiation, which deeply moves the imagination. It may too be safely affirmed, that Dr Henry's habits of extreme mental accuracy, his unrivalled manual expertness, and the general tendencies of his tastes towards elegance and precision, peculiarly qualified him to excel in conducting such delicate enquiries.

But although his original efforts in chemistry were mainly directed to the gases, he was by no means a stranger to other provinces of experimental research. His comparative analysis of many varieties of British and Foreign salt, were important in refuting the prejudices, then widely prevalent, of the superiority of the latter for certain economical purposes. His essay on the Materiality of Heat, published in the memoirs of this learned body, is written with force and perspicuity, though in modern times the tide of doctrine undoubtedly sets in the opposite direction. His critical memoir on the theories of galvanic decomposition has even lately been noticed with praise by Berzelius in his Jahrsbericht, as among the first to assert that view, which in his judgment is still most deserving of adoption. But of the soundness and extent of Dr Henry's acquaintance with general chemistry, his "Elements" furnish undoubted proof. This work, which in 1829 had passed through eleven editions, has always held a high place in public estimation, as a clear and faithful record of the details of chemistry, and as conveying, in its general chapters, a distinct and comprehensive survey of the leading doctrines and higher philosophy of the science.

Although impelled by a strong predilection to the pursuit of chemical science, Dr Henry was not an inactive member of the profession to which he belonged. Besides contributing to the medical journals of the time, such interesting cases of disease as fell under his observation, as physician to the Manchester Infirmary and other public charities, he engaged in an elaborate investigation of that important class of maladies, which affect the urinary system,—the exact diagnosis of which is well known to rest on indications purely chemical. His inaugural discourse on uric acid; his analysis of many varieties of calculi; and his essay on Diabetes, were favourably received by the profession, and are still cited with approbation by our best pathologists. Even after having relinquished the exercise of medicine, he continued to feel a deep interest in its advancement, and on a late occasion when the ravages of Asiatic cholera in neighbouring countries suggested the necessity of preventive measures on our own coasts, he established by experiments, as satisfactory perhaps as the nature of the enquiry admitted, the destructibility of various contagious poisons, by degrees of heat inferior to the boiling point of water. It is due, however, to his philosophical caution to state, that Dr Henry regarded these experiments only as initiatory, and as demanding the confirmation of multiplied and varied trials, before being adopted as the basis of legislative enactments. His feelings of interest had been so deeply excited in the laws and higher physiology of contagion, that he embodied all the facts and evidences he had been able to glean from a most extensive course of reading, in a report which was communicated to the British Association, and has been published in their transactions.

Dr Henry's compass of thought and interest was not, however, restrained within the limits of his profession and his favourite branch of knowledge. Of the sciences of classification, he had cultivated in early life, with great zeal, both botany and mineralogy; and had formed a creditable collection of mineral specimens. This latter study naturally led him into the kindred pursuit of geology. Indeed, his first academical residence in Edinburgh coincided with the memorable period, when the two rival theories were the objects of constant and eager controversy in all societies, and especially in the higher scientific circle, in which Dr Henry had the privilege of moving. Shortly after the formation of the Geological Society of London, Dr Henry was admitted a Fellow; and though he never aspired to collect by personal research, materials for the advancement of that science, yet he diligently possessed himself of all that was successively made known, and deeply sympathized in its prosperous fortunes and high destinies. During the latter years of his life, especially, these pursuits ministered largely to his sources of enjoyment, and prompted him to undertake several short journeys, with the object of examining interesting sections, and of collecting characteristic fossils. The growing literature of a science, that has attracted to itself so large a share of the intellect and genius of this country, replaced as an object of interest in Dr Henry's mind, the contemporary progress of chemistry, from the details of which, in consequence of physical inability for experimenting, he had ceased to derive much pleasure.

In polite letters, Dr Henry had ever been accustomed to seek variety and relaxation from severer study. His range of interest was singularly comprehensive. He took peculiar delight in narratives of voyages and travels, and from such works was in the habit of gathering and preserving all novel facts that tended to throw light on the physical history of the earth, or the manners and mental habitudes of its inhabitants. He has thus strongly expressed his sense of the value and dignity of such personal labours and perils in the cause of science. "No subject within the compass of human knowledge embraces so wide a sphere of enquiry, or so much tends to gratify an enlightened and liberal curiosity, as voyages and travels undertaken with a right aim, and by persons qualified to reap their rich and varied fruits. To those engaged in them, are offered all the fascination of novelty, all the hopes of wider and brighter prospects of the moral and natural world; all the warm impulses of an honourable ambition to live beyond the present times, and to be remembered by conquests more glorious and more useful than those of the field. These high motives kindle and keep alive a spirit which sustains them through toils, difficulties, and disappointments, and enables them to triumph over physical privations and pains, which would dishearten the stoutest, if encountered in the every day transactions of life." Biography, especially that of men devoted to the pursuits of philosophy, always occupied him most agreeably,—carrying back his intellectual sympathies to distant periods, and supplying him with materials for afterthought and speculation. His mind had been early nurtured with the choicest fruits of our national poetry :—and the same purity of taste and affections, which in music made him peculiarly accessible to the simpler melodies, guided him to the fresh and gentle beauties of our earlier poets. He often commended as a happy imitation of their manner, " the Castle of Indolence," a poem, which he more than once read to his domestic circle, with that delicacy and truthfulness of intonation which are inspired only by deep and intuitive perceptions of excellence.

In determining the literary merits of the works of others, and still more in the expression of his own thoughts, Dr Henry was guided by a correct, or rather a severe taste, which might have rendered him over-fastidious ; had not his critical judgments been attempered by the fervour of his sympathies, and by the comprehensiveness of his mental vision. An enemy of redundancy in expression or in ornament, he erased in the vigilant revisals, through which all his compositions had to pass, every superfluous term, reaching finally as complete condensation of style as was consistent with ease and distinctness. In strictly philosophical writing he was frugal from principle in the employment of imagery ; aiming solely at the simple and logical enunciation of truth. But in his literary essays ; in his biographical notices, when warmed by the contemplation of genius or virtue ; and especially in his letters, when his feelings had been touched by the works of nature, or when surveying the grand lines and general bearings of science, and shaping forth his future course, as a philosophical enquirer or writer, his style received embellishment and warmth from a powerful yet chastised imagination, and from a heart prone to generous and noble emotions. His eloquent delineation of the intellectual features of his great contemporaries, Davy and Wollaston ; —his enthusiastic homage to the soaring and creative genius of Davy, and his no less truthful picture of the opposite endowments,—the caution, the sobriety and precision of Wollaston, are probably fresh in the minds of many present ; and may recal Mr Playfair's celebrated contrast of Black and Hutton, both in many qualities common to the minds compared, and in the vigour which characterizes alike both comparisons. An earlier essay by Dr Henry, entitled Cursory Remarks on

Music, may also be commended as a fine example of the gracefulness and purity of his style, when handling topics of elegant letters.

Dr Henry appears indeed to have been eminently fitted both by natural tastes and by after culture, to excel in what may be called the literature of science ; comprehending especially under that term, the history of discovery and the didactic exposition of general laws and doctrines. In his latter years, he seems himself to have strongly felt, in the perception of growing infirmities, that his season of active research was gone by, and to have looked around for some worthy object, not demanding personal exertions, to occupy what remained to him of life and of mental strength. His thoughts had dwelt for some time on two scientific projects, for both of which he had made considerable preparations. One of these designs, which had floated longest before his mind, and which he was most inclined to realize, was a work that should assemble the beneficent provisions in the Chemical Economy of Nature, which establish the existence and attributes of an All-wise Governor of the Material Universe. He thus expressed, in a letter addressed many years ago to the writer, his conceptions of the scope and dignity of such an undertaking. " It has always appeared to me a defect in physico-theological works, that too frequent appeals are made to the reason, in proof of divine wisdom, and that their efficacy is weakened instead of being confirmed by needless iteration. It is enough if a writer, on a subject full of these proofs, presents them first apart from each other, and then in combination, in clear, plain, and unaffected language, to the understanding of the reader, and contents himself with a general but forcible impulse towards the conclusions respecting their causation, which have forced themselves upon his own mind. A work of this kind, executed as it ought to be, would be a foundation for a just reputation to its author, and for a more durable one than can be raised by any abstract of the state of technical chemistry, which, however well executed at the time, must soon be rendered obsolete by the rapid march of discovery, while the great and leading principles of chemical philosophy will stand unimpeached and unchanged landmarks to guide those who are in search of truth. There would too, I think, be great utility in such a work, because, independently of all such tendency as that to which I have alluded, it would place the reader on a station from whence he might enjoy a distinct view of the surrounding world, of that world with which he is brought closely into contact, and with which he is every hour conversant, but whose most beautiful arrangements he passes unheeded by. What a wide field of phenomena, for instance, admit of explanation by the laws respecting heat;—the effects of its expansive power both on bodies themselves, and in rendering them its vehicles to distant regions, borne on the waters and the air, which envelope our globe—the influence of the provision of latent heat in retaining it in great storehouses, where it is felt only for good and from whence it issues in continued and vivifying abundance when the sun withdraws his warmer beams—all that relates to the radiation of caloric through infinite space, and its reception by the subjects of the mineral, the vegetable, and the animal kingdoms;—the admirable contrivances especially, by which the latter are cheered and animated, without injury even to the most delicately organized. Surely these are topics (and they are but a very small portion of the whole) on which no man can expatiate without that pure delight, which truth first breaking through ignorance or error, sheds over the mind, refining and exalting both our moral and intellectual natures."

The other literary project, for which Dr Henry had also collected some materials, was a history of chemical discovery from the middle of the last century, and devoted in largest measure to the glorious epoch of Scheele, Cavendish, Black, Priestly, and Lavoisier. As the historian of his favourite science, it was Dr Henry's design to have pursued the method so successfully traced by Sir James Mackintosh in his invaluable dissertation on Ethical Philosophy—that of developing the progressive advances of the science through the lives and triumphs of its most eminent cultivators. The biographical notice of Dr Priestly, made public in the first volume of the Reports of the British Association, was to have formed one of this gallery of historical portraitures. Such objects, and especially the calm retrospect of the advances of knowledge, and the deliberate estimate of the services of genius, Dr Henry conceived to be the appropriate employment of advancing years, which, while they chill the active energies of invention and creation, ripen the judgment and incline to contemplative habits, to which they minister the accumulated materials of past study and experience. The evening of life, he often remarked, was far from ungenial to maturity or even vigour in composition, and he readily assented to the similar sentiments so eloquently enforced by Sir James Mackintosh, when characterizing the autumnal fruits of Mr Stewart's genius. It is matter of deep regret, that Dr Henry was not permitted to execute this great design; for, as a writer, it may safely be pronounced that he was never more happy than in his power of discriminating the finer intellectual distinctions, and of painting vivid yet not overcharged mental resemblances. Maintaining an enlarged communion with all orders of intellectual greatness, and an enthusiastic worshipper of genius in all its manifestations, he delighted in thus offering to it his fervent homage, and in giving worthy expression to the intenseness of his feelings and convictions, and to the ardour of his sympathy in every discovery that promised to advance the well-being of mankind, and to further the cause of universal truth and science.

To the members of the Society, who as a body have already placed on record their affectionate respect for his memory, and with some of whom he had maintained throughout life an unbroken friendship, cemented by kindred tastes and mutual esteem, it can scarcely be necessary to offer any detailed portraiture of his moral excellencies. Yet there were some traits rather of manner than of character, which by those not in habits of close intercourse with him, might not, perhaps, have been always rightly interpreted. Thus there was occasionally a reserve of manner that might be regarded as implying coldness of feeling, but which arose solely out of the languor produced by an almost constant state of bodily indisposition, and increased by those habits of studious application from which he could never be induced to relax. Though not liable to acute maladies or to such as seemed to endanger life, he had to struggle with what is perhaps less supportable, an habitual infirmity of health and feelings of oppression arising from the slow and imperfect action of the digestive functions. These distempered sensations he was accustomed to lament, mainly as abridging his season for intellectual labour, and especially as disqualifying him for original thought and composition.

In the general intercourse of society, Dr Henry was distinguished by a polished courtesy, by an intuitive propriety, and by a considerate forethought and respect for the feelings and opinions of others;—qualities issuing out of the same highly-toned sensibility, that guided in taste his letters, and that softened and elevated his whole moral frame and bearing. His comprehensive range of thought and knowledge, his proneness to general speculation in contradistinction to detail, his ready command of the refinement of language, and the liveliness of his feelings and imagination, rendered him a most instructive and engaging companion. To the young, and more especially to such as gave evidence of a taste for liberal studies, his manner was peculiarly kind and encouraging. He was most anxious to promote, as far as was in his power, their progress in knowledge, and on one occasion cheerfully dedicated a considerable portion of time to initiate some young friends in those more refined operations of analysis in which he was so consummately skilled.

From this imperfect record of Dr Henry's original labours in science, and of his tastes in letters and in philosophy, a more faithful impression of his intellectual habitudes and endowments may perhaps be gathered, than from any general mental analysis. In science, it will have appeared that his efforts are mainly characterized by ingenuity and elegance in devising instruments and methods of research, and by extreme skill and precision in their employment. But in measuring the amount and importance of Dr Henry's contributions to chemical knowledge, it must be borne in mind that in his season of greatest mental activity, he never enjoyed that uncontrolled command of time, and that serene concentration of thought, which are essential to the completion of great scientific designs. His intellectual seed-time was encroached upon by the duties of an extensive medical practice, and by other equally pressing avocations, and his experimental inquiries were conducted at late hours or at intervals snatched from engrossing pursuits and with the liability to constant interruptions. In more advanced life, when relieved from such exertions, growing infirmities, and failing bodily power, restrained him to studies not demanding personal exertion, and even abridged his season of purely mental labour. That, amid circumstances so unfriendly to original and sustained achievements in science, he should have accomplished so much, bears testimony to that energy of resolve, that unsubdued ardour of spirit, which ever glowed within him, urging him steadily onwards in the career of honourable ambition and prompting exertions more than commensurate with the decaying forces of a frame that had never been vigorous.

Though, moreover, the science of chemistry undoubtedly held the highest place in Dr Henry's sphere of knowledge and interest, any measure of the strength and compass of his mind, which should rest simply on his chemical acquirements and discoveries, would be eminently inadequate. In forming such estimate, it is essential to his just intellectual station, that regard should be had to the soundness and extent of his knowledge in various branches of physical and natural science, in the advancement of which he had no design of actively participating, and to which he was attracted by no other motive than the delight he experienced in the varied exercise of his faculties, and in the perception of new truths. Nor would it be just to overlook his rare endowments as a philosophical thinker and writer, the clearness and fidelity with which he assembled and methodized the scattered fruits of discovery, and the simplicity and vigour which characterize his exposition of general doctrines. It would appear, indeed, from some slight notices of his early occupations, which are still preserved, that, at the very outset of his career, he had projected a scheme of study remarkable for its comprehensiveness; having probably, even then, arrived at the conviction, that an equable development of the various faculties, active as well as speculative, is most conducive to sound mental discipline and to individual well-being; and that a commanding survey of the kingdoms of nature, and enlarged sympathies with the creations of human thought and genius, are wisely purchased by the sacrifice, if necessary, of a somewhat higher degree of excellence in a single department of knowledge.

In conclusion, it may be permitted to one so near to him in blood and in affection, to indulge the conviction, that faculties so vigorous and excursive, so amply furnished with materials for the illustration and enforcement of truth, might, had they been reposited in a less frail tenement, have raised some enduring memorial of their compass and energy;—if dedicated to the history of science, impartially weighing and recording the services of individual minds, yet with vigilant and subordinate reference to the general intellectual movement of each epoch; from which even genius itself derives its primitive impulse if not its special direction;—or if aspiring to trace the footsteps of design in the Economy of Nature, ascending from the loftiest generalizations and most comprehensive laws to the contemplation of the Great Fountain of all truth and of all science.

Analyses of Basalt and of Lava from Etna. By A. Löwe of Vienna.*

Basalt has often been made the subject of chemical examination. Of the analyses, in which it was regarded as a simple mineral, those of Klaproth and R. Kennedy are particularly deserving of attention. The former examined the basalt of Hasenberg near Klappey, not far from Libochowitz in Bohemia, and the latter that of the island of Staffa.

These analyses afforded the following results:—

	From Hasenberg.	From Staffa.
Silica,	44.50	48
Alumina,	16.75	16
Oxide of iron,	20.00	16
Lime,	9.50	9
Magnesia,	2.25	—
Oxide of manganese,	0.12	—
Soda,	2.60	4
Muriatic acid,	—	1
Water and volatile matter,	2.00	5
	97.72	99

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* From *Poggendorff's Annalen*, 1836.

More recently, Berzelius, in his Memoir on Meteoric Stones,[*] and C. M. Gmelin, in his Examination of Phonolites,[†] have pointed out a method which may lead to the enlargement of our chemical knowledge of compound minerals, and which consists in first separating them into two portions by treatment with acids, when the one is decomposed and gelatinized, while the other remains undecomposed, and then subjecting each portion separately to analysis.

In this manner C. M. Gmelin analyzed different varieties of basalt,[‡] and found one portion to be decomposable in acids and the other undecomposable; and that the one was distinguishable from the other by the circumstance of the former containing potash, soda, and water, while the latter approached augite in its composition.

The following are the results of his analysis:—

a. Basalt from Stettin, a basaltic conical hill in Hegau.		
Quantity of the gelatinized mass .	61.54	per cent.
.... ungelatinized mass	38.46	...
b. Basalt from Hohenstoffeln in Hegau.		
Quantity of the gelatinized mass .	61.97	...
.... ungelatinized mass	38.03	...
c. Basalt from Sternberg near Urach.		
Quantity of the gelatinized mass .	87.72	...
.... ungelatinized mass	12.28	...
d. Basalt from the district of Wezlar.		
Quantity of the gelatinized mass in the undecomposed basalt . .	40.29	...
.... weathered portion .	23.73	...
.... ungelatinized mass in the undecomposed basalt .	59.71	...
.... weathered portion .	76.27	...

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[*] *Poggendorff's Annalen*, vol. xxxiii. p. 10.

[†] Transactions of the Würtemberg Society, vol. ii. p. 133, and *Poggendorff's Annalen*, vol. xiv. p. 357.

[‡] *Leonhard's Basalt Gebilde*, part i. p. 266, &c.

	a			
	Gelatinised mass.	Ungelatinised mass.	As a whole.	Gelatinised mass without black oxide of iron (*eisen-oxydul oxyd*).
Silica, . .	35.741	48.500	40.64	44.50
Titanic acid, .	——	——	——	——
Alumina, .	11.121	6.792	9.57	13.85
Black oxide of iron,	16.015	——	13.35	——
Peroxide of iron,	——	9.383	——	——
Oxide of manganese,	1.487	0.436	1.10	——
Lime, . .	11.914	17.395	14.02	14.83
Strontia, .	0.112	——	0.07	0.14
Magnesia, .	10.434	13.131	11.47	13.00
Soda, . .	3.264	——	2.01	4.06
Potash, . .	1.204	——	0.74	1.49
Water, . .	6.530	——	4.01	8.13
	97.822	95.637	96.98	100.00

	b	*c*	*d*	
	Gelatinised mass.	Gelatinised mass.	Gelatinised mass.	Ungelatinised mass.
Silica, . .	35.13	36.94	28.91	56.65
Titanic acid, .	——	——	6.63	——
Alumina, .	12.24	10.58	11.64	9.16
Black oxide of iron,	18.30	——	28.79	——
Peroxide of iron,	——	13.34	——	3.99
Oxide of manganese,	1.70	0.30	0.21	——
Lime, . .	8.08	14.18	7.37	12.65
Strontia, . .	——	——	——	——
Magnesia, .	13.17	11.04	5.46	3.91
Soda, . .	3.05	3.30	3.67	3.17
Potash, . .	1.91	2.46	1.50	3.13
Water, . .	4.59	3.59	5.32	——
	98.17	95.73	99.50	92.66

In order to ascertain if the basalts of other districts have a similar composition, I undertook the analysis of a very characteristic Silesian variety, which was kindly given me by Professor G. Rose, who also permitted me to perform the process in his laboratory. The locality of the specimen employed was Wickerstein, near Querbach, in Lower Silesia. Its colour was greyish-black, and it contained pretty large crystals of augite, together with imbedded grains of magnetic iron-ore. The crystals of augite were easily distinguished from the basis by their higher lustre; and were made particularly apparent on the decomposed surface, where the basis was rendered white and earthy by the weathering, and was removed by the atmospheric water, while the imbedded crystals being better calculated to resist such action, were not destroyed. The rock was compact, and the fracture uneven. Small fragments heated in a small glass retort, gave off water, which produced no reaction on litmus paper. The loss by heating amounted to 3.95 per cent.; but in all the analyses performed, the quantity of hygroscopic water amounted to nearly 1.25 per cent., so that, after subtracting the last from the total loss by heating, the true quantity of water which can be calculated among the constituent parts of basalt is 2.70 per cent. After being heated the basalt had an altered aspect: the basis became reddish, and the crystals of augite which had remained unchanged, had become very perceptible. The powder of fresh portions of this basalt was greenish-grey, similar to powdered black augite: it formed with muriatic acid, in a short time, a perfectly firm jelly; and the same result was obtained with fragments that were heated and finely divided.

With the magnet only an extremely small portion could be detached from the coarse powder, and this was perceptible in the form of a delicate covering on the edges of the magnetic bar. Thin splinters, held by platinum pincers, and acted on by the blowpipe, communicated a yellow tinge to the outer flame, and were melted at the edges to a black obsidian-like glass. Some experiments proved to me that by the application of diluted muriatic acid, the basis or the gelatinizing component part is fully decomposed, while the magnetic iron is hardly at all acted on; and that the augitic portion itself, by long continued action of a concentrated acid, is partially dissolved.

By means of muriatic acid of ordinary strength, the soluble portion was wholly decomposed. The silica was separated from the insoluble portion by boiling with a concentrated solution of carbonate of soda. An attempt to accomplish this in the mechanical way afforded an unsatisfactory result.

The course pursued in the analysis was such as is generally followed in the case of combinations which are partially decomposable by acids, and which on the other hand can be only decomposed into their constituent parts by fusion with alkaline carbonates.

The actual loss by heating, after subtracting the hygroscopic water, can be assigned only to the basis, that is the gelatinizing portion; for it is well known that the magnetic iron-ore and the augite contain no water. The iron contained in the soluble portion was considered in the calculation as a black oxide.

In the investigation conducted in this manner, 6.0775 grm. of basalt were reduced to the finest powder; and of this quantity 3.3782 grm. were insoluble in muriatic acid. The soluble portion therefore amounted to 2.6993 grm. The silica amounted to 0.9465 grm. The alumina and oxide of iron were precipitated by ammonia, and separated by caustic potash; the alumina was determined by means of carbonate of ammonia, while the oxide of iron was determined by succinate of ammonia, the former amounting to 0.7015 grm., and the latter to 0.2905, which, when calculated as black oxide, yielded 0.2805 grm. The lime precipitated by oxalic acid yielded 0.2546 grm., and the saline residue obtained by evaporation to perfect dryness, was found to contain 0.3367 soda and 0.0348 potash.

Of the insoluble portion 2.428 grm. were fused with three to four times the weight of carbonate of soda. The silica thus obtained weighed 1.165 grm.; the alumina and oxide of iron, separated as above, amounted respectively to 0.221 and 0.401 grm. (the latter considered as black oxide); the lime = 0.350 grm.; and the magnesia ascertained by means of carbonate of potash, as well as by phosphate of soda, gave as the amount 0.315, including a trace of manganese. We have therefore for the soluble portion the following composition:—

		Oxygen.
Silica, . .	35.06	18.21
Alumina, . .	25.98	12.13
Black oxide of iron, .	10.39	4.08
Lime, . .	9.43	2.64
Soda, . .	12.47	3.19
Potash, . .	1.28	0.21
Water, . .	6.07	5.39
	100.68	

Leaving out the black oxide of iron, which is to be regarded as a disseminated magnetic iron-ore, we have the following proportions of the constituent parts:—

		Oxygen.
Silica,	39.13	20.32
Alumina,	29.00	13.54
Lime,	10.52	2.95
Soda,	13.92	3.56
Potash,	1.43	0.24
Water,	7.93	7.08
	100.93	

From this result we obtain the following formula:—

$$\left.\begin{matrix}\dot{C}^{3}\\ \dot{N}a^{3}\\ \dot{K}^{3}\end{matrix}\right\} \dddot{S}i + 2\,\dddot{A}l\,Si + 3\,\dot{H}.^{*}$$

The gelatinizing portion is zeolitic according to its chemical composition, but does not agree with any known zeolite; it approaches most nearly to the Thompsonite, the formula of which is probably—

$$\left.\begin{matrix}\dot{N}a^{3}\\ \dot{C}a^{3}\end{matrix}\right\} \dddot{S}i + 3\,\dddot{A}l\,\dddot{S}i + 6\,\dot{H},$$

but which exhibits a different proportion of the alumina to the lime and soda, and also a different quantity of water. It may also be regarded as a zoisite containing water, and in which a part of the lime is replaced by soda and potash.

The non-gelatinizing portion contains—

		Oxygen.
Silica,	47.98	24.92
Alumina,	9.10	4.25
Protoxide of iron,	16.51	3.75
Lime,	14.41	4.04
Magnesia and a trace of manganese,	12.97	5.02
	100.97	

This analysis shews the composition of the augite; but a formula cannot easily be deduced from it, and in this respect it agrees perfectly with the results obtained by Kudernatsch.[†] The examination confirms in another way what was apparent to the unaided eye.

By calculating the composition of basalt from the first and second analyses, we obtain the following result:—

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[*] In each of the two formulæ given in this page, the A and the H ought to have additional lines drawn through their centres, but we find that our types do not admit of this.

[†] *Poggendorff's Annalen*, vol. xxxvii. p. 577.

		Oxygen.
Silica, . .	41.18	21.39
Alumina, . .	17.39	8.12
Black oxide of iron, .	5.15	1.45
Protoxide of iron, .	8.18	1.86
Lime, . .	11.82	3.32
Soda, . .	6.18	1.58
Magnesia, . .	6.43	2.48
Potash, . .	0.63	0.10
Water, . .	2.70	2.40
	99.66	

From this no further conclusion can be drawn; but the preceding analysis proves that the basalt from Wickenstein is a compound of crystallized augite with a compact zeolitic substance and disseminated magnetic iron-ore. Of these component parts there were contained in the fragment analyzed:—

Augite, ...	55.58
Zeolitic mineral, . . .	39.81
Magnetic iron-ore, . . .	4.61
	100.00

The lava from Etna consists of a grey-coloured basis, in which are imbedded small greyish-white tabular crystals of a felspathic substance, blackish-green crystals of augite, and small grains of olivine. Professor G. Rose has determined this felspar-like portion to be Labradorite, from its re-entering angle on the most distinct cleavage faces, and from its being accompanied by augite.* It occurs in the lava in large quantity; the augite is found more sparingly, and the olivine is still less abundant. The lava is from the stream which in 1669 destroyed a great part of Catania; the portion analyzed was obtained from the collection of the late Professor F. Hoffmann.

The decomposition of this lava was accomplished in the same manner as in the basalt. The gelatinized mass was to the ungelatinized as 24.89 to 75.11. Repeated experiments convinced me that no loss of weight resulted from a long continued red heat; the external aspect remained unchanged.

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* *Poggendorff's Annalen*, vol. xxxiv. p. 29. Von Buch and Elie de Beaumont are also of opinion that the felspathic component part of this lava is Labradorite, vol. xxxvii. p. 189.

The gelatinizing portion contains :—

		Oxygen.
Silica, . . .	43.31	22.49
Alumina, . .	12.83	5.99
Protoxide of iron, .	26.86	6.11
Lime, . .	7.48	2.10
Soda, . .	6.94	1.77
Potash, . .	1.56	0.26
	98.98	

The non-gelatinizing portion :—

		Oxygen.
Silica, . .	54.76	28.44
Alumina, . .	19.62	9.16
Lime, . .	11.22	3.15
Protoxide of iron, .	5.93	1.35
Magnesia, . .	9.21	3.56
Protoxide of manganese,	1.09	0.24
	101.83	

The lava as a whole therefore contains :—

		Oxygen.
Silica, . .	48.83	25.36
Alumina, . .	16.15	7.54
Protoxide of iron, .	16.32	3.71
Lime, . .	9.31	2.61
Magnesia, . .	4.58	1.77
Soda, . .	3.45	0.88
Potash, . .	0.77	0.13
Protoxide of manganese,	0.54	0.12
	99.95	

The analyses afford no result from which an accurate conclusion can be formed regarding the composition of the lava ; it is probable that more than one component part has a share in the solubility in acids, and that thus the different portions cannot be calculated separately. But the circumstance is worthy of notice, that lava, a genuine volcanic product, does not contain a trace of water, while basalt has never been found without water. Kennedy pointed out the existence of soda in the lava of Etna so early as the year 1798, in his Memoir in the Transactions of the Royal Society of Edinburgh.

Remarks upon the Maculla Hot Spring in Arabia, together with some Notes regarding the Red Sea Islands. By W. Hibbert, M. D., Assistant-Surgeon of 2d or Queen's Royal Regiment.*

The few observations treated of in the present communication, were made during a voyage to India by the overland route, and at a period when I was considerably pushed for time, —a circumstance I consider it necessary to mention, as it will in some measure account for the very superficial view I could only take of the country, my stay being then limited to a few days.

The impression which instantly strikes an observer upon entering the Bay, which is rather a superior one, is the utter barrenness and nakedness, devoid, one might say, of every thing interesting, and moreover so sterile, that there absolutely does not exist sufficient herbage for a few sheep, those, as well as other domestic animals, being invariably fed upon animal food, and chiefly salt fish, a diet which, we well know, renders them by no means acceptable to an English palate. The effect of such treatment also, appears to affect their growth considerably, as few sheep in this quarter are known to exceed eight pounds in weight. Proceeding, however, a few miles inland, some cocoa-nut trees slightly vary the scene, with also a few ruined huts which present themselves, and some wretched watch-towers on the neighbouring hills.

From this slight deviation, I shall now proceed to the more direct object of the communication, by giving a hasty sketch of the rocks in the neighbourhood, as also the properties of the accompanying water, which was taken, while warm, from the Maculla hot spring, and sealed immediately afterwards.

The hills have rather a peaked form, and a structure chiefly amygdaloidal, with but scanty herbage, and this consisting principally of the following: Senna, Cactus, Euphorbium, Indigo,

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* Communicated by Sir James M^c^Grigor, Bart., M. D. Director-General Army Medical Department, &c. &c.

Lavender, &c., and also in the valleys we observed a few cultivated cocoa-nut trees and cotton plants, the latter the *Gossypium herbaceum*, being a diminutive and but inferior article. The former, I may remark, thrive particularly well around the hot spring, although it is not in the slightest degree brackish, and moreover is situated about three miles up the country, and perfectly sheltered from the sea-air.

I never met with any fossil remains in the neighbourhood. The rocks upon landing consist of a fundamental one of a green clay limestone, surmounted and continued along either side of the coast in a S.E. direction, and the superincumbent the same, with an amygdaloidal structure. Extending the walk farther inwards, beds of littoral alluvium were passed over, formed of agglutinated animal remains, as shells, corals, &c. &c., apparently of late date, and shewing here and there the latter rock beneath. Reascending, the former rock was again met with, and continued so far as the hot spring; and on tracing the spring to its source, I found a subjacent compound formation of ochre, ironstone, and sandstone. I have little doubt that the spring passes over this formation and also across magnesian limestone, and thus receives some impregnation from both of them.

The surrounding hills, which are numerous and similarly formed, appeared to me to be composed of like rocks, chiefly limestone, although, in the valleys, I here and there picked up small pieces of granite, syenite, &c., but which I think must have come from a distance in the interior, as I never traced any rock of that description in the vicinity. It is the water here alluded to, which, when cooled, is drunk by the natives, and notwithstanding its slightly magnesian and calcareous taste, is relished, and is certainly by no means insipid. I ascertained the temperature by the thermometer to be 98½°, when it issues from the rock, forming a very agreeable warm bath, much resorted to by the natives on this account, and amongst them adopted as a last resource in obstinate chronic diseases, particularly cutaneous, tedious cases of dysentery and fever, diseases which are most prevalent in this neighbourhood; in short, they have actually gone so far as to build a reservoir for the purpose, at the bottom of which the thick calcareous deposit which has taken place may be seen. I ascertained that hot springs similar to this, but which I had not an opportunity of visiting, existed also at a place several miles to the eastward, and also at Muscat, which is about 1½ days' journey inland. As is usually the case, I think there can be little doubt as to the existence, and that not very far distant, of some internal fire, a circumstance which is in a measure supported by the knowledge we possess of other parts of the coast,* as well as from

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* The circumstances alluded to above refer to the known existence of a recently extinct volcano, and also volcanic remains in the small islands and coral rocks, so numerous and notoriously dangerous on the Arabian side of the Red Sea.

The most interesting of these, from the circumstance of its having been seen burning so late as 1833 and 1834, as well as being the largest, is the island called *Jebel Teir* or *Tor*. At the time we saw it some smoke was visible, occasioned by sulphureous exhalations similar to those daily occurring at Vesuvius, and some of the neighbouring phenomena, as Solfatara, &c. It is composed of ancient as well as modern volcanic rocks, in some parts, having a stratified appearance, as if of layers of tufa and lava, and at the highest point was an immense quantity of sulphur, which had been thrown out at a somewhat late period. The summit contained a crater.

Situated in a still lower latitude are the seven hills, or *Jebal Sebar* or *Saabe*, all equally of volcanic origin, of ancient date, and forming some very fine craters, one in particular, receiving the name of the basin island, from its very perfect form. A few also are curious from their precipitous appearance, and of course all of ancient volcanic rocks. In short, the only modern one in this respect is the Jebel Teir or Tor above mentioned.

It may be remarked, that all the rocks in this sea shelve very precipitously, in many no ground being found at 240 fathoms even at the edge, thus exemplifying their perpendicular height. These are all volcanic, as also are all those from Jidda to the Straits; but above the former place, they are chiefly coral, yet equally precipitous on all sides. Arkeeko, N. Lat. 15° 33′, E. Long. 39° 31′, may be cited as an other interesting remain, from the beautiful and safe haven it affords, being a mere ring of rocks, deficient only at one small spot, serving as an entrance to the bay.

It may also be interesting to note, that many coral rocks occur in this quarter besides those in other places, quite close to what are undoubtedly volcanic. Moreover, that an immense or even unfathomable depth is equally found close to the one as to the other, which circumstance would lead to the conclusion of its not being unlikely that what we call coral rocks, are merely incrustations of corals upon volcanic rocks, which had never reached the surface.

A circumstance, and in favour of the opinion of many geologists, perhaps it the evidence of our countryman Mr Wollf, the Abyssinian traveller and missionary, who, in his travels to Senaar in February last, mentioned to me that he was told an active volcano at present existed about four days' journey from this very spot. The assertion, of course, originating with an Arab, is not to be hastily adopted; but, nevertheless, it would not be surprising, taking collateral circumstances into consideration. Moreover, were not this the case, that is, their occurrence in an otherwise non-volcanic district, it is well known that some also are associated with granite and other rocks, to which most assign an igneous origin, and consequently the same causes produce similar effects. An excellent example of such is to be found in the upper part of the Jumna, in the now Agra Presidency, where the springs issue from rocks of granite with a temperature of 194° F., and other similar instances in the same and

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may not be trespassing too far to take notice of here, that is, that in Lat. 18°, on the east coast of the Red Sea, there is at present a coral rock 120 feet above the surface, the very summit of which, and the greater part of the side, is crowned by corals upon a volcanic foundation, which one would certainly imagine, and not incorrectly, must have been raised by some eruption whilst the process of incrustation by these animals was going on,—an evidence, moreover, which perfectly sides with the previous hypothesis, that almost all the coral rocks in this sea are formed upon a similar basis. This, I am aware, is objected to by many, who are of opinion that the alternate rising and falling of the sea is amply sufficient to account for such an occurrence.

Now, this rising and falling of the sea cannot, in this case, account for the phenomenon in question, since in no quarter of the globe are we aware of the tide rising to a height exceeding 100 feet, viz. in the Bay of Fundy, and even this is doubtful; and here the highest is never known to reach one-half of the above.

From this, therefore, we might almost come to the conclusion, that what are denominated coral rocks here, and perhaps even in other parts of the globe, are in fact nothing more than the remains of extinct volcanoes, or other rocks incrusted by these animals, and by degrees raised to their present height, a summary which might probably be resolved into the four following stages, viz. Volcanic rocks naked and above the surface of the sea; the same naked above the surface, incrusted with coral below; thirdly, incrustations below the surface, upon a supposed volcanic basis; and, fourthly, Volcanic rocks with coral incrustations above the surface.

It may be stated, that, for the fact above alluded to with regard to the soundings, I am indebted to the kindness of Lieutenant Campbell of H. E. I. C. S.

other presidencies might be cited. Now, the existence of similar rocks in this neighbourhood I believe I have already alluded to, as also the proximity of extinct volcanoes, and consequently an intimation of a derangement of strata as an additional evidence is unnecessary; and even should none of these exist, there may be igneous rocks below the limestone, which, as I before mentioned, exists in great quantity, and so impregnates the various springs as to lead to the formation of stalactites, resembling exactly those at Knaresborough, and incrusting substances in a similar manner. Over and above this, as an additional proof, it might perhaps not be out of place to bring forward the origin of springs; from the circumstance of fresh-water occurring so near its source, the ocean, where so very little humidity in the atmosphere is known, shewing that filtration, capillary attraction, or some subterraneous heat causing it to rise in vapour through fissures, collect and issue in springs (which is the most probable and certain), is the cause of this purification of the water. Strong objections, and amongst these Dr Halley's stand prominent, are, I am aware, started against these theories, such as no upward filtration or current by capillary attraction, and so on; yet the latter, by collecting in cavities, might at least be sufficient to produce intermitting springs. The last theory, however, upon the whole, is the most probable and likely, and only allows of one objection, viz. the ceasing of springs on some occasions in dry weather; yet, I would not be understood to say all are produced from the sea, but that many, very many, must, and are to be accounted for in that way. Neither do I conceive all cases equally apply to that of Dr Halley, that is, rain falling on higher ground, filtrating and issuing at a lower level, as if so, how account for springs and a permanent flow of such on or near the summit of hills where neither rain nor clouds are known, as in Upper Egypt; or how, for the but slight brackishness, if no effect from either filtration or internal heat, the former of which, I am of opinion, must also have some effect. This one might attempt to account for by saying, as is said in dry weather, that the water collects in underground caverns, and filtrating slowly, gets into the springs. But one naturally asks, from whence the water to fill these caverns is derived? not

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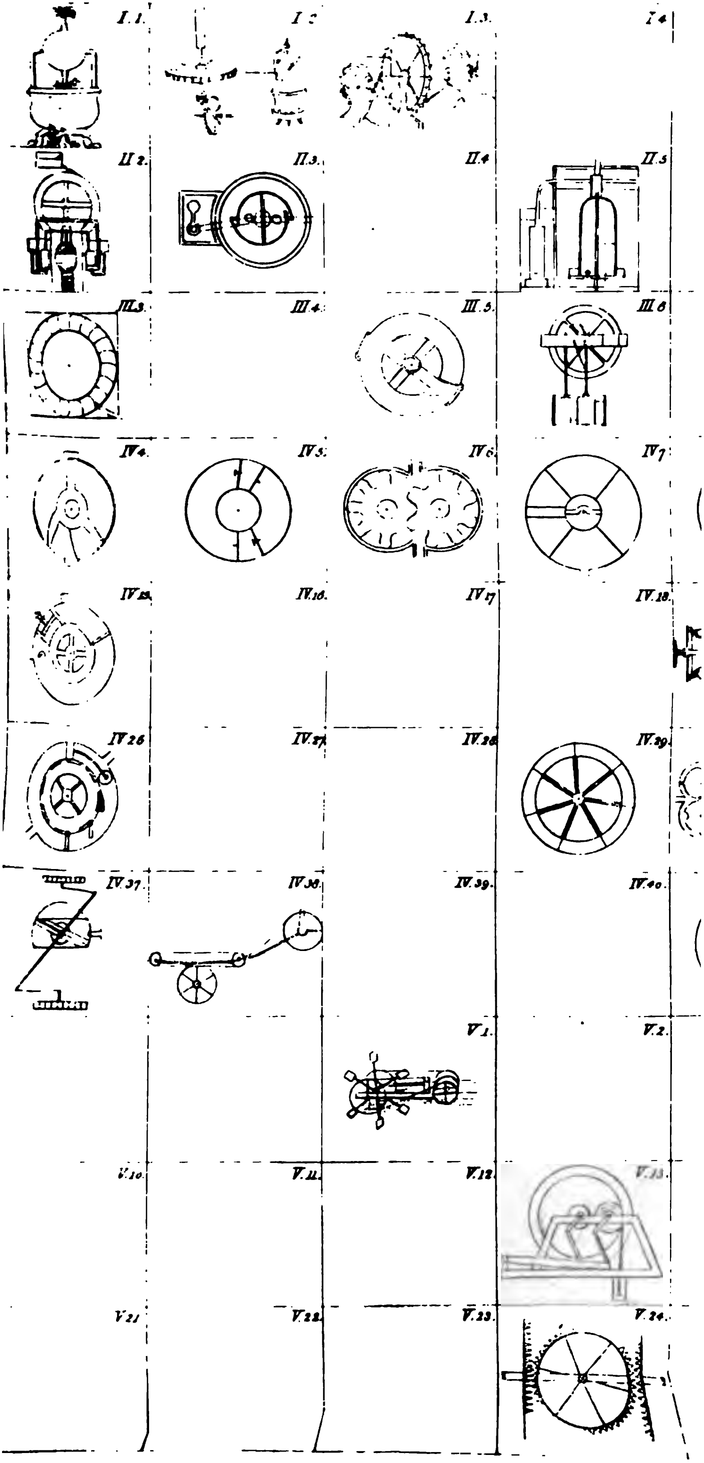

from the clouds, nor yet rain, as the atmosphere contains none; nor yet can we plausibly refer it to the Nile, as between this spring, which is nearer the sea, and the above river, there are brackish as well as other fresh-water springs, therefore, only from the sea itself, and this issuing through igneous rocks, which is actually the case, is purified, and that by the subterraneous heat assisted by filtration, whilst the former pass through Neptunean rocks; occurrences which one would at first be apt to consider as decisive. Indeed, it only remains to shew, whether rock-salt, &c. exist in this neighbourhood (near Kenek), to set the point at rest, but the presence of which, being rather pushed for time, I never was so successful as to ascertain, and therefore must leave it for some future traveller to determine, as well as intrust to some ingenious chemist the analysis of the accompanying mineral water, since my present station is very far from affording me the means of conducting scientific investigations.

BOMBAY, *May* 1837.

On the Fallacies of the Rotatory Steam-Engine.[*] By JOHN SCOTT RUSSELL, Esq., M.A., F.R.S. Ed., Lecturer on Natural Philosophy.

IT has been represented to me by the Secretary, that the objects of this Society will be materially promoted by any disquisition in which the fallacious views that are sometimes entertained upon important mechanical subjects shall be clearly analyzed, and the errors pointed out into which the authors of supposed improvements have been drawn, either by reasoning accurately on false grounds, or making erroneous deductions from established principles. I feel it, therefore, to be my duty to make such contributions to the efforts of this most valuable Association, as my humble abilities enable me to produce; and I have selected for this purpose the Rotatory Steam-Engine, as a subject upon which erroneous views are widely prevalent, upon which much ingenuity and mechanical skill is every day expended, and which belongs to the same category of fallacies to which the Quadrature of the Circle and the Perpetual Motion have long been assigned.

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[*] Read before the Society of Arts for Scotland, 15th March 1837.

To any one who compares the state of the Mechanical Arts in Great Britain at this instant, with their condition at the commencement of the century, the progress of these arts will undoubtedly appear more rapid in their approach to perfection, and more extensive in their range of application, than during any former period in the history of civilization; but if he will direct his attention more closely to these wonderful effects, and, looking below the surface of events, will examine into their causes, if he will consider how large and wealthy a portion of our population have directed their whole talents and energies solely to the purpose of attaining perfection in these arts, he will be disposed to question whether the results have been at all proportioned to the means, and whether by such mighty interests judiciously directed, more would not have been achieved, had these resources been devoted exclusively to legitimate problems of real improvement, instead of being expended on the ignes fatui of mere visionary speculations, and had the talents which have been permitted to deviate from their proper channel been devoted to such ends only as should permanently benefit society, and form decided steps in the advancement of civilization, or valuable additions to the truths of science.

To direct the enterprise and resources of one part of this empire into the legitimate avenues of valuable improvement, and to afford the means of distinction and encouragement to the mechanical talent of Scotland, is the object of this Society; and when I reflect how many men there are, even within the limited sphere of my acquaintance, whose inventive genius is of the highest order, and whose labours are yet abortive, I cannot but feel convinced that there must be either a misapplication of talent to objects which others with better opportunities, and possessed of peculiar advantages, have better accomplished, or a misdirection to subjects containing in their own nature something either impracticable or impossible. Were it possible on the other hand, for such men to unite their exertions for promoting the real advantage of society, and were every individual, by a proper division of mental labour, to direct his mind to the object most congenial to it, I cannot imagine but that, with such means so directed, changes and improvements in the state of the arts would be produced in this country much more rapid and astonishing than all that we have already witnessed.

I am led to make these general remarks by their applicability to a series of inventions which have successively appeared under the generic appellation of *Rotatory Steam-Engines.* Their principle has assumed various forms and modifications, and has seduced, and still continues to seduce, many a bright genius from the straight path of useful industry and accurate invention. I have the pleasure of personal acquaintance with several men of eminent talent, who have sacrificed the energies of great minds to this ruinous fallacy. With one or two, my arguments have been successful in dissuading them from a pursuit sure to end in disappointment, but there still remain others of them, and many beyond the sphere of my knowledge, of whose talents and exertions the world is still deprived by the *fallacies of the rotatory steam-engine.*

It is the object of this paper to shew that the whole principles of the rotatory engine, as an improvement upon the common reciprocating engine, whether condensing or noncondensing, is radically false, and mechanically fallacious: that it is false in its mathematical principles, fallacious as a mechanical structure, and can never be attended with any mercantile advantage in its application; and thus to dissuade men of mechanical talent from devoting themselves to so unworthy an object.

Before requesting acquiescence to be given to me on any opinion so decided as this, I ought to premise, for the purpose of obtaining the confidence of practical men, that, although the views which I am about to develope were first suggested to me in the course of an investigation where I found it necessary to bring the battery of the higher analysis to bear upon this subject, and employ the powers of the calculus to raze the foundations of prevalent error in the steam-engine; yet, as such men are apt to use the word "practical experience," as antithetical to scientific skill, I ought to mention that, during the last ten years, I have been continually engaged in the practical solution of the most difficult problems of the steam-engine,—that I myself invented, and had constructed for me, several rotatory engines, which were sufficiently successful to convince me that *the principle*, and not the mere *application* I had made of the rotatory principle, was radically wrong. I have also had the opportunity of examining and working the most successful engines of this kind ever produced, and therefore conclude that had theory never led me to any such result *a priori*, I must have been convinced that practical experience was opposed to the rotatory construction of the steam-engine. In what follows I shall endeavour to adduce my arguments in a form as little technical as is consistent with precision.

1. It is first of all my wish to shew that the subject of the rotatory steam-engine is *not so new and untried* an invention as some who attempt the problem for the first time may be led to imagine;—for this purpose I adduce the names of more than ninety inventors, most of them patentees.

2. By an arrangement of these inventions, I have endeavoured to shew that *five different classes comprehend them all*, and that the others are mere repetitions of the same principle, and attended with the same failure; so that an inventor may know whether his invention contains an entirely new principle, and if it do not, that it has already been tried and failed.

3. By shewing, in one view, the names of inventors of unsuccessful rotatory engines, I endeavour to convince the inventor that the five classes already invented have *not failed from want of genius, skill, or practical experience*, in those who have made the trial, for the list contains the names of eminent practical men.

4. I endeavour to shew that the *ordinary crank-engine does not possess the defects attributed to it*, and which it is the sole object of the rotatory engine to remedy,—that the use of the crank causes no loss of power.

5. In a practical point of view, the *rotatory engine is every way inferior* to the reciprocating engine;—in simplicity, and cheapness, and ease of construction,—in durability and economy in use,—in uniformity of action and equable motion.

6. The rotatory engine is *peculiarly inapplicable to the great purposes* of terrestrial locomotion and steam navigation—objects to which it has been considered peculiarly suitable.

7. That the *present steam-engine is practically perfect* as a working machine, being within ten per cent. of mathematical perfection.

8. That the *crank* of the common steam-engine possesses cer-

tain *remarkable properties* of adaptation to the nature of matter, of motion, of steam, and the human mind; from which its supremacy as an elementary machine is derived,—properties which cannot possibly belong to any species of rotatory engine.

The common or reciprocating steam-engine is distinguished from the rotatory steam-engine by the nature of certain parts of its mechanism, which convey the motion of the steam to the machinery which is to be moved. There are a cylinder, a piston-rod, and a crank-axle. Now the root of the whole of the fallacies of the rotatory engine will be found in certain radical misconceptions of the nature of the crank, as the simple elementary instrument by which the revolving motion of the axis, or great wheel of the steam-engine, is immediately produced. Nothing can be simpler than the crank. We wish to turn an axle round; we bend a part of the end of it at right angles to itself; we take hold of this end, and by this means turn round the axle. The bent part is the crank, and may be seen every day in winding up a clock, or in turning any wheel on its axle, by holding a spoke; likewise in the handle of a coffee-mill, at the top of a draw-well, or in the handle of any winch or crane for raising weights. Now, in the same manner as the hand of the operator takes hold of the handle, and, by drawing it towards him or pushing it from him, makes the axle or wheel turn round,—so does a rod from the piston of a steam-engine take hold of the end of a crank, and alternately draw and push it round in its circle of revolution. The crank is indeed so simple that it can scarcely be called an addition to the axle of which it forms a part; it is merely a bend or *crook* in it, which the word *crank* originally implies, and has been used to move the pistons of the cylinders of common pumps, since the days of Aleotti, in precisely the same way as it now moves in the steam-engine. Now, it is owing to a radical misconception of the nature of this elementary machine, that hosts of schemes have arisen for the production of circular motion, without the intervention of the crank, either by giving to the steam itself an immediate circular action, or by the substitution of some other less elementary mechanism, between the reciprocating piston and the revolving axis, as the means of producing its rotation. In the rotatory engine, on the other hand, the cylinder, piston, rod, and cranked axle are superseded, in the most common form, by a cylinder, valves, stop, and axis. In the same way as a mill-wheel is compelled to move in a circle, either by the direct action of water or wind upon it, so is the drum or wheel, with valves, fans, or other projections on its circumference, urged round by the force of the steam, and, enclosed in an outer cylinder or case, gives revolution to an axis to which it is attached. This direct rotating action of the steam will, it is imagined, give out its effect more powerfully, uniformly, and economically, than the common mode of reciprocating action, when converted by the crank into revolution.

Rotatory engines may be arranged according to the mode of action into four classes.

Class I.—Rotatory engines of Simple Emission.

Class II.—Rotatory engines of Medial Effect.

Class III.—Rotatory engines of Hydrostatical Reaction.

Class IV.—Rotatory engines of the Revolving Piston. As closely connected with the rotatory engines in the fallacy which has given rise to both of them, we may add a series of inventions forming a

Class V.—Revolving Mechanism substituted for the Crank.

Class I.—The rotatory engine of Simple Emission forms the earliest, as well as the most rude and elementary method of giving motion to mechanism, by the escape of vapour or steam. It is described by Hero of Alexandria, in his *Pneumatika*, upwards of 120 years before Christ, and depends, for its effect, upon the same principle which gives to a rocket its career, and makes a fire-wheel revolve in giving off its beautiful lights. In these, as in all instances where fire, or steam, or any fluid or gas is generated in a chamber from which it is permitted to issue with violence, it will, in its exit, drive the vessel from which it issues away from it in the opposite direction, and is, in fact, merely an application of the principle of recoil,—where the gas, generated by the explosion of the powder, urges the ball outwards in one direction, and forces the breech of the gun backwards in the opposite one. The same recoil is felt in all cases of simple emission of a fluid from a reservoir; and if it be so arranged that water, steam, air, or the gaseous productions of gunpowder, shall rush out of a chamber through the arms of a revolving wheel, the openings of escape being properly directed, the recoil will urge round the wheel, and we shall have a revolving engine of Simple Emission. By availing himself of this principle, the Machinist of Alexandria produced an efficient engine, merely by heating a vessel containing water and air, and allowing the vapour to rush from two opposite orifices at the end of two arms proceeding from a sphere which the emission was employed to move.

Instead of using the principle of recoil, the force of steam issuing with violence as we see it from the mouth of a kettle or boiler, may be directed upon the vanes of a wheel, so as to blow them round; and thus we have a second variety in the manner of converting the simple issue of steam into a moving power. This second species of the rotatory steam-engine of simple emission was invented by Branca in 1629.

Since that time the engines of this class have been frequently re-invented and slightly modified.

Inventors of Rotatory Steam-Engines of the First Class. See Pl. I. Figs. 1–8.

1. Hero of Alexandria,	B. C. 130.	5. Kempel, .	A. D. 1785.
2. Branca, .	A. D. 1629.	6. James Sadler,	... 1791.
3. Kircher, .	... 1643.	7. Richard Trevithic,	... 1813.
4. Daslesme, .	... 1699.	8. Alexander Craig,	... 1834.

The theory of machines of simple emission has been frequently and fully investigated, and the result is, that there is no possibility of obtaining, by simple emission, more than one-half of the whole power of the steam, so as to make it available to useful mechanical effect. The other half is wasted in giving off its impulsion to the air, or is expended in a current equally unavailing.

Practical experience corroborates the predictions of theory. Smeaton and Pelletan made the machines of simple issue the subject of careful experiment, and found that 3 parts out of 11, 8 parts out of 27, and 2 parts out of 5, are the highest measures of practical effect that it has been found practicable to attain, and by no possible improvement can more than one-half of the whole power be turned to a useful effect.

Class II.—Rotatory engines of Medial Effect are those which do not immediately give revolution to an axis by the action of steam upon the wheel, but have a *medium of communication* between the power and the effect, which medium is the direct agent in circular motion. This class of engines will be well understood by taking as its type any simple steam machine, such as Savary's and Newcomen's, used for raising water, which water by falling on the floats of a common mill-wheel will then give rotatory motion to it. The engine of Savary raises water by pressing directly on its surface, and it is only necessary to allow this water to fall on a wheel, when it will be made to revolve and form an engine of the 2d class.

A variety of this class has been invented of which the fire-wheel of Amontons is a type. The stream pushes water through certain channels that form the arms of the wheel, from a set of chambers on one side of the wheel, to a corresponding set of chambers on the opposite; and thus the side filled with water preponderates over the other, and the wheel revolves. The water being constantly driven off by the steam from a given side of the wheel to that opposite, uniform revolution is the result of the weight of the water. In this state, although steam is the agent, water is the medium of communicating the rotatory motion. See Plate I. Fig. 11.

Solids have also been made the medium of effecting rotation in this manner; weights of solid mater, in the form of pistons, have been transferred by the force of steam to a considerable distance from the centre on one side of a wheel, and drawn nearer to it on the other side, so as, by bringing about a continual preponderance on one side, to effect revolution. Watt and Witty have designed rotative mechanism of this nature.

Inventors of Rotatory Steam-Engines of the Second Class. See Pl. I. Figs. 11–20.

1.	Guillaume Amontons,	A. D. 1699	6.	Richard Witty,	A. D. 1810
2.	Leopold of Plainitz,	... 1723	7.	Sir W. Congreve,	... 1818
3.	Champion of Bristol,	... 1762	8.	John Moore, .	... 1820
4.	James Watt, .	... 1769	9.	Sir W. Congreve,	... 1821
5.	Davidson & Hawkesley,	... 1793	10.	Thomas Masterman,	... 1822

In this class of engines the loss of effect is manifest, for it is necessary that the steam, in order to produce circular motion, shall give out its force in setting the medium in motion, and in overcoming the very great resistance of the liquid in all the pipes, passages, and valves through which it is transmitted to alternate sides of the wheel in every revolution: the whole of this force is subtracted from useful effect, and becomes power lost.

In those which move weights from and towards the circumference, there are mere groups of reciprocating pistons without cranks, and share the evils to be explained in Class V.; in fact, in the engines of Watt and Witty of this class, we have *a series* of reciprocating engines ranged round a wheel to do the work of one.

In the case of the fluid medium, we have not only a loss of all the power expended in moving the medium itself, but also the additional loss of effect encountered in all modes hitherto adopted for applying a fluid to the rotation of a wheel, a loss, in the best examples ever presented, amounting of itself to more than one-sixth part of the power.

Class III.—The Engine of Hydrostatical Reaction is more effective than either of the former classes. As invented by Watt in 1769, it consisted of steam-vessels in the form of hollow rings or circular channels, with proper inlets and outlets for the steam, mounted on horizontal axles, like the wheels and buckets of a water-mill, and wholly immersed in some fluid. This wheel was made of iron, six feet in diameter, and the reaction of mercury was employed to give revolution to it; the engine moved, but was found to be inefficient and abandoned, although it had been tried in very favourable circumstances.

The principle of action is this: Steam is admitted into a circular channel or chamber on the circumference of a wheel; this chamber is partially filled with some liquid, the pressure of the steam is expended in pushing the mercury in one direction, and the end of the chamber in the opposite way, so that while the liquid is thus forced out of the chamber, the chamber is by an equal force pushed away from the liquid; the wheel is thus turned round.

It is apparent that a part of the force is employed in propelling the wheel, and the remainder is expended in overcoming the resistance of the liquid of reaction, and expelling it from the chambers, which remainder is a large portion of the power withdrawn from useful effect.

Inventors of Rotatory Steam-Engines of Third Class. See Pl. I. Figs. (21–30).

James Watt,	A. D. 1782.	Bryan Donkin,	A. D. 1803.

Class IV.—Rotatory engines, having a Revolving Piston, are constructed on a much better principle, and hold out much fairer prospects of successful competition with those having the ordinary reciprocating piston, than any of the species of the three first classes that have been already considered. In these classes the steam is not confined in rigid vessels, but its action is applied to producing currents in fluids, and force is expended in medial effects which are useless, and therefore waste power. This is not the case in the steam-engine of the revolving piston. The steam is confined in a close and rigid chamber, and acts only on a solid inflexible surface, and makes its escape by confined passages, so that its full effect may be obtained in useful work. Abstractly considered, it is an engine capable of giving out the full power of the steam, and therefore may fairly be imagined to come into competition with the ordinary reciprocating crank engine. The objections to it are entirely of a practical nature, and regard the engine not in its abstract mathematical form, but as a machine made of destructible matter, of matter imperfectly elastic, of surfaces opposing resistance to motion, of matter obeying the known laws of motion and rest. These objections are not the less valid that they are of a sensible and tangible, rather than a speculative description. But as a natural consequence of the more plausible deceptions held out by this species than by any of the three preceding ones, it has followed, that the fallacies of this class have been more widely seductive than any of the others. The Patent-office presents us with the names of more than forty victims, including some of the highest fame.

Inventors of Rotatory Steam-Engines of the Fourth Class. See Pl. I. Figs. 31–80.

1. James Watt,	A. D. 1782	4. Edmund Cartwright,	A.D. 1797
2. James Cooke,	... 1787	5. Jonathan Hornblower,	... 1805
3. Bramah & Dickinson,	... 1790	6. William Murdoch,	... 1805

7. Jonathan Hornblower,	A.D. 1805
8. John Trotter,	1805
9. Andrew Flint,	1805
10. William Lester,	1806
11. Richard Wilcox,	1806
12. Thomas Read,	1808
13. Edward Jane,	1808
14. Samuel Clegg,	1809
15. William Chapman,	1810
16. John Trotter.	1811
17. William Onions,	1811
18. Richard Trevithic,	1813
19. Joseph Turner,	1816
20. John Mallam,	1818
21. Joshua Routledge,	1818
22. William Carter,	1818
23. John Rider,	1820
24. Robert Delap,	1821
25. Bambridge and Thayer.	1821
26. William Foreman,	1824
27. Lord Cochrane,	A. D. 1825
28. L. M. Wright,	1826
29. F. Halliday,	1825
30. Joseph Eve,	1825
31. John Costigin,	1826
32. Marquis de Combis,	1826
33. Elijah Galloway,	1826
34. Paul Steenstrup,	1827
35. John Evans,	1828
36. John Strut,	1830
37. E. and J. Dakeigne,	1830
38. William Morgan,	1831
39. Samuel Hobday,	1831
40. John Erricson,	1832
41. Robert Stein,	1833
42. Elijah Galloway,	1834
43. Edward Appleby,	1835
44. John F. Kingston,	1835
45. John Yule,	1836
46. John White,	1836

The fallacy of this class of engines we shall expose in conjunction with the next class, as the same misconceptions lie to a considerable extent at the root of both.

Class V.—Revolving Mechanism substituted for the crank of the common steam-engine, for the purpose of obtaining from the reciprocating piston a rotatory effect otherwise than by the crank, and in a better manner than by the crank, forms a class of inventions involving fallacies similar to those in which the revolving piston has originated. These two may therefore be considered together.

Inventors of Rotatory Mechanism as a substitute for the Crank.
See Pl. I. Figs. 81–110.

1. Jonathan Hulls,	1737
2. Keane Fitzgerald,	1757
3. Gautier of Nancy,	1757
4. John Stewart,	1769
5. Dugald Clarke,	1769
6. Matthew Washborough,	1779
7. James Watt,	1781
8. Thomas Burgess,	1789
9. Matthew Murray,	1779
10. William Lander,	1799
11. Phineas Crowther,	1800
12. George Medhurst,	1801
13. Edmund Cartwright,	1801
14. Matthew Murray,	1802
15. Richard Witty,	1811
16. J. Dawes,	1816
17. Tobias Mitchell,	1817
18. Henry Penneck,	1820
17. William Aldersey,	1821
20. Robert Barlow,	1827
21. Thomas Peck,	1827
22. Samuel Clegg,	1828
23. William Lucy,	1836
24. Charles Schafhautl,	1836

Although the name of Watt has been included in this list of inventors of substitutes for the crank, it should be observed, that he was only driven to the invention of such a substitute by the circumstance of a patent having been previously obtained for the crank in its simple form, and that he abandoned his beautiful, but more complex, mechanism on the instant that the elementary crank was released from the fetters of monopoly. It is also due to his memory to say, that the sun and planet wheel he substituted for the crank, is a disguised crank, and possesses all the valuable properties, excepting simplicity and smallness of friction, which give to the crank its present eminence as a source of rotatory effect. Although, therefore, he was an inventor of rotatory mechanism, he is by no means to be regarded as a victim to the fallacies of which other substitutes have been the offspring.

In exposing the nature of the fallacies of the two last classes of inventions, we shall avail ourselves of the accounts of their misconceptions on the subject of the common reciprocating crank engine, which formed the ground of the preference given by the inventors of these improvements to their own mechanism, as these conceptions have been given by the inventors themselves, and those who have adopted their view of the subject. Fortunately for us they have been particularly full in explaining their views.

Thomas Masterman of Ratcliffe, patentee of a rotatory engine says, in recommendation of his engine—

"The importance of an effective and economical rotatory steam-engine will not be controverted, when it is recollected that the steam-engine on the reciprocating principle *absorbs about half the power of the steam.*"

"William Aldersey of Homerton, in the county of Middlesex, gentleman," in his patent substitute for the crank, observes that—

"The object of this invention is to equalize the motion, and principally to save the *force which is unnecessarily thrown away* in steam-engines, and all other machines in which a reciprocating or backward and forward motion is converted into a circular one; and the simple manner in which it is here effected, will, it is presumed, be found a great desideratum by all who have occasion to use such machines, since a saving of power is a pecuniary saving of coals, which, in the constant use of a machine, is of much more importance than its first cost. That this saving is really effected in the present machine, will be sufficiently evident to those who possess mechanical knowledge; and for the conviction of those who do not, I have subjoined the diagram by which the action of the ordinary crank may be demonstrated. If the crank rod be so placed as to act either from above or below, it will have no power at all (figs. 1 and 3.) to turn the crank round, while it is, in either of the two situations where the crank and rod are in one line, as at A and B, but it will have the greatest possible power upon it at those instants when it is at two other intermediate points (M and N, figs. 2 and 4), the consequence of which is, that while the power forces the crank to revolve, it increases in the first quarter revolution from zero to a maximum, and diminishes in the second quarter from the maximum to zero, and so on for each half revolution. The loss of power attendant on the use of the crank, is found in figures to be nearly one-third of the whole."

Fig. 1.

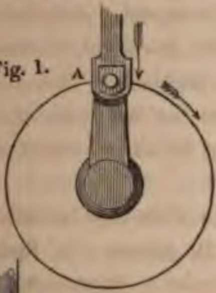


Fig. 2.

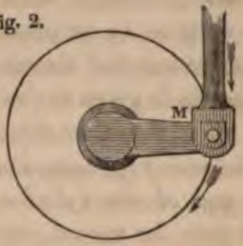


Fig. 4.

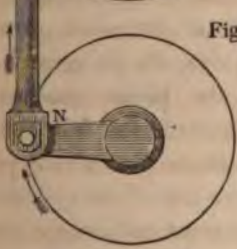
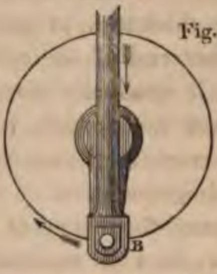


Fig. 3.



Mr Aldersey expresses clearly the sentiments of a host of inventors, all of them pursuing the main object of doing away the imaginary loss of power occasioned by the crank. I am sorry to add, that some eminent standard writers on the steam-engine have advanced the same doctrines.

I shall now, then, proceed to shew that the pressure produced by the crank is not a loss of dynamical force; that, where the apparent loss is greatest, there is no loss at all, and that at every point, the effect produced is directly proportioned to the quantity of steam producing it.

Let it be recollected that at the two extremes of the line of the centres, the greatest *apparent* loss of force takes place.

But is it not seen that at this instant there is in reality no loss at all, because there is no expansion of steam, no consumption of the elements? The supply has been closed, the communication with the boiler is cut off, the steam has done its work, and only waits to be dismissed from the chamber, which it leaves on the instant that the eductive valve is thrown open for egress, and access is given to the fresh supply which is forcing its way inwards to the other side of the piston.

The entering steam finds the piston almost in contact with that end of the cylinder at which it enters. It insinuates itself into the vacant disk, and powerfully expanding swells out its thickness, propelling the disk of the piston towards the other end of the cylinder. At first its progress is but slow; it gradually accelerates till the piston reaches the middle of the cylinder, when it moves with the full velocity of the crank in the centre. But, as the motion of the piston must altogether cease on arriving at the end of the cylinder, it is prepared for this event in a manner most exquisitely beautiful; for, from the moment of reaching the point which is half way from the end of its course, the piston begins to be retarded, the steam expands more and more slowly, the final stoppage is gradually prepared for, and at last the rectilineal motion having dwindled to nothing by insensible shades, altogether ceases; the steam has expanded by a continually diminishing movement and now ceases to produce any effect, and is released from its confinement. Such is the history of the elemental power in its transit through the cylinder, first communicating motion to the piston by gradually increasing increments, and then bringing it to rest, by decrements of motion, in the inverse order.

Let us now trace, with equal minuteness, the simultaneous phenomena of the point, which, by its connection with the piston, is carried round in the circumference of a circle, while its mover progresses in its reciprocal strokes in the cylinder. How is this gradual change from rest to motion, and from motion to rest, to be rendered consistent with uniform motion communicated to the crank in its circle? it is not only consistent with it, but consequent upon it. It is to this very graduation of rectilineal motion, that the uniform circular motion contributes, and it is with such alone that it could be any degree consistent.

To trace the simultaneous positions of the point describing the circle of the crank, and the point describing the straight line in the cylinder, it is necessary that we have recourse to a simple diagram. You are requested to conceive a circle with its centre and radius placed at a given distance from the end of the cylinder, so that a piston moving in the given straight line of its piston-rod, at a given distance from it, is so connected with it that the motion of the one produces the motion of the other.

Let Figs. 5 and 6 represent a cylinder, and the Circles 1, 5, 10, 15, and 20, the path of the crank, the numerals on the cylinder and on the circles representing corresponding places of the crank and the piston at given instants of time. In Fig. 5, the motion of the crank is supposed uniform, and the circle divided into equal parts, while in Fig. 6 the axis of the cylinder is supposed to be divided into equal parts to represent a hypo-

Fig. 5.

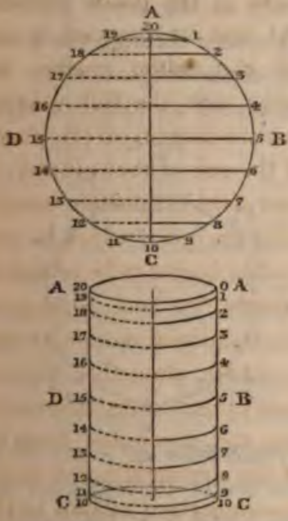
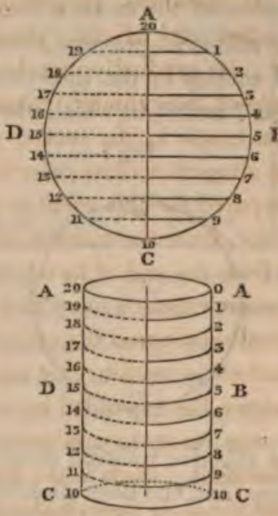


Fig. 6.



thetical case of uniform reciprocating motion in the piston; so that the piston, when elevated and depressed by the steam, draws the crank down with it from A to B and C, and raises it up with it from C to D and A. Let the orbit of the crank be divided into twenty equal parts, the first ten numerals being placed on the descending side, and the other ten on the ascending side of the circle. Let the length of the stroke of the piston, which is equal to the diameter of the circle, be divided into ten parts, shewing the place of the piston at every instant of its stroke, contemporaneous with the points of the crank in its orbit; the first ten numerals corresponding both in the circle and the cylinder with corresponding points of reciprocation and of revolution.

Now, at each of these points, let us examine into the relation which exists between the pressure of the steam on the piston and the quantity of the effect produced in revolving the crank, and that part of the pressure of the steam which produces no motion, and which is therefore erroneously said to be lost. The pressure of the steam acting obliquely on the crank produces two effects;—pressure towards the centre through the crank, and motion in the direction of the circle tangentically. See Figs. 7 and 8, in which the arrows placed as tangents represent effective rotating force, the vertical lines the power of the steam, and the lines directed to the centre the apparent loss. Let the whole pressure of the steam be taken at 100 lb., then, by ascertaining the proportion which the lines *a*, *b*, and *c*, in the direction of those parts bear to each other, it will be seen that, at the first division, the force in the direction of the circle is only 31 per cent. of the force on the piston; at the second division it will be found to amount to (58.78) 59 per cent; at the third to (80.90) 81 per cent.; at the fourth to (95.11)

Fig. 7.

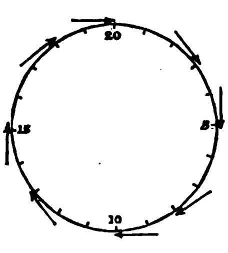
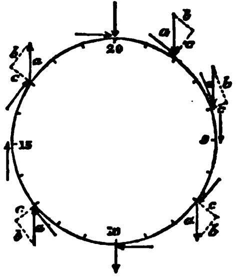


Fig. 8.



95 per cent.; and at the fifth division, having completed one quarter of a revolution, the pressure is for an instant equal to 100 per cent., or the whole power on the piston; but the action has now become a maximum. In its next point a diminution of circular force takes place: at the sixth division the force has diminished by 5 per cent.; at the seventh 19 per cent. are lost (apparently); at the eighth 41 per cent.; at the ninth 69 per per cent. are lost; and at the tenth the piston has reached the end of its stroke, and the whole pressure applied to the crank, in this position, would produce no effect at all on the circular orbit, or the apparent loss would be 100 per cent. Equal diminutions take place through the remaining semicircle during the returning stroke of the ascending piston.

The following table of the part of the power directly available to the production of circular motion at ten points in the semicircle, may therefore be found.

Points in the Fig.	Arc moved over by Crank.	Force in the direction of revolution.	Relative velocity of Crank to Piston.
0 and 20	0°	0.00	Infinite.
1 ... 19	18	30.90	3.236
2 ... 18	36	58.78	1.701
3 ... 17	54	80.90	1.236
4 ... 16	72	95.11	1.051
5 ... 15	90	100.00	1.000
6 ... 14	108	95.11	1.051
7 ... 13	126	80.90	1.236
8 ... 12	144	58.78	1.701
9 ... 11	162	30.90	3.231
10 ... 10	180	Mean 63.138	Infinite.

Such is the reasoning by which, with apparently correct steps, many have been led to the conclusion, that, in the crank, the mean effect is less (by more than one-third) than the pressure exerted in the cylinder by means of the piston.

I have gone into an analysis of this false reasoning the more fully, as it puts more perfectly before the mind of the reader the circumstances in which the error of the conclusion lurks in concealment. I shall proceed immediately to lay open the source of error, and the means of arriving at the truth.

Let it then be recollected, that, in all calculations of power, we must attend to *space passed over*, as well as to the force exerted in that space. And that a force of *two* pounds, moving a weight of two pounds through *three* feet a second, is equal in effect to a force of *three* pounds moving a weight of three pounds through a space of only *two* feet a second; or that, in calculating the quantity of effective power, a greater velocity is equivalent to a greater force. Thus much being premised of the principle of virtual velocities, as applied to uniform motions, if we find that, in the case of the crank, the force acting on the piston produces an effect through the crank, in which, although the force may be less, it is exerted through a space equivalently greater than that through which the piston is moved, then we have a right to infer that there is no loss of power, but that what is lost in force is gained in velocity. Now, this is precisely the case.

While the piston moves through the length of the cylinder, which is equal to the diameter of the crank circle, it moves the crank through the circumference of a semicircle; now, the length of the diameter is to the length of the semicircumference as follows : Diam. :—semicirc. : : 2 : 3.14159, being a little more than the ratio of 63 : 100.

If, then, we find that the mean force exerted through the crank is not less than in this proportion, we shall be able to shew that the loss is imaginary. Now, the mean force on the crank obtained from the third column of the preceding table is also 63 to 100; that is to say, *the force in the piston is greater than the mean force in the crank in precisely the same ratio in which its velocity is less than it.*

There is another view of the matter to which I would direct particular attention. It is said that, at the top and bottom of the stroke, the position of the crank is so unfavourable to the production of circular motion, that no force of steam, however great, would produce any good effect towards causing revolution. This leads me to trace the progress of the steam itself in the cylinder, in connection with the motion of the piston and of the crank.

The steam, when the piston is at the top of the cylinder, has not yet been admitted to press upon the piston, and, therefore, none of its force can at this moment be lost. On the instant of leaving this point, the steam enters, and at the end of the first 18°, presses round the crank with a force of only 12.80 per cent.; but let it be observed, that the steam only carries the piston through a space of about 1–8th part of that through which the crank revolves,—so that, in producing this effect, the motion of the piston is just so much slower than the motion of the crank, as the power on the crank is less than that on the piston; so that here again we have a dynamical equivalent of a greater force and a slower motion to a higher velocity with a less force.

As the piston descends and the pressure increases, it will be observed that the velocity of the piston increases exactly in the ratio of its effect on the crank; so that, at the instant when the pressure on the crank and piston is equal, their velocities are also equal, and during the subsequent decrease of pressure on the crank, the velocity of the piston diminishes in the same ratio. The motion of the piston is not therefore an uniform but a variable motion, its velocity varying according to the pressure on the crank; and the two dynamical effects are not only equal in ultimatum, but they are equal at every instant of time; and if, on the other hand, the motion of the piston had been uniform, the motion of the crank must have been a variable quantity, as it is represented in Fig. 6, a case which does not occur, and would be unsuitable at once to the nature and laws of matter, and to the practical application of mechanical power.

It appears therefore, 1. That the mean pressure on the crank during the whole revolution, is less than the pressure on the piston just in the proportion in which the space moved over by the latter is less than the space described by the former, so that the total of all the power in the one is equal to the total of all the effects in the other; 2. That the steam is not at all expended at the neutral points, and that its expenditure is at every point exactly proportioned to the power it gives out in useful effect; 3. That the velocity of the motion of the piston is in the ratio of the force acting at each instant on the crank.

These conclusions, here derived from obvious and elementary views of the relations of position and velocity, are originally deduced from the same data, by the more rigid method of the calculus. In the numerical exemplification I have endeavoured to give here of that more correct process, I have been compelled to select only a few points in the circuit, and the result is only approximately correct; but as the same might obviously be done at any point, the result is equally satisfactory.

The study of the higher analysis cannot be too strongly recommended to those who wish to avoid errors in invention.

In the reciprocating piston, therefore, acting through the crank, the whole power is found by multiplying the stroke and back-stroke, or twice the stroke of the piston, by the pressure upon it, and this is equivalent to the whole effect produced in the entire revolution of the crank; the pressure of the steam, and the space it moves through, are therefore the measure of the power.

In the revolving piston, the effect of the steam must be precisely the same, if the revolving piston be of the same size, and moves through the same space as the reciprocating piston; and if the revolving piston have a pressure on it equal to the mean pressure on the crank, and move through a circle equal to the circle of the crank, the effect will be the same in both cases.

Since there is no loss incurred by propagating the action of the steam on a reciprocating piston through the crank of a revolving axle, and since it is not in the power of machinery of any kind to augment the quantity of power given out by any mover, but merely to arrange, dispose, and modify that power to suit any given purpose, it follows that the rotatory piston can have no purpose to accomplish, unless it excel the reciprocating one in simplicity and economy of construction, diminished bulk, durability and economy in operation, facility of repair when deranged, diminished friction, or a peculiarity of adaptation to some individual purpose, such as steam navigation or inland transport.

I. As regards simplicity of parts, the engine with the rotatory piston cannot excel the simplest forms of the reciprocating engine; take, for example, that form which merely consists of a cylinder, piston, and crank axle; where the cylinder, mounted on an axle, oscillates with the revolutions of the crank, which is immediately attached to the end of the piston rod, and which requires no moving valves of any kind, the steam being admitted and emitted through ports in the axle of the cylinder, which open and shut by the motion of the cylinder itself. Neither as regards facility and economy of construction does it possess superiority; for it will be readily granted to me, that whether the piston and channel in which it moves be rectangular or circular, they are more difficult of construction than a straight, round cylinder and piston, which, being derived from the straight line and circle, are the simplest of forms, while an annular chamber, if the piston be rotatory, is a surface of double curvature, of difficult construction and imperfect completion; if square, the construction of a rectangular piston is a still more troublesome attempt, the increased surface being increased expense and labour.

II. In point of bulk, the common reciprocating piston has a decided advantage; the annular cylinder of the revolving piston must be (to give equal power) about two-thirds of the area, and about three times the length of the reciprocating cylinder, being a bulk of cylinder nearly double. But even this is an estimate too favourable to the piston of rotation; the diameter of the axle requires to be very considerable; there are various reasons for this,—one is, that it is frequently a steam-passage; another is, that it is much larger than is required for the mere purpose of communicating the force, because any force of steam applied near the centre is of little value in producing an effect, from the smallness of the circle which that part of the piston describes; and for this reason also, that the portion of piston exposed to leakage and wear is in proportion to the effect gained; the piston is therefore removed to a considerable distance from the centre, to answer the purpose likewise of rendering the revolution of the parts more nearly equal. These points will, however, have our attention at another time; it is sufficient for our present purpose, that these circumstances render it imperative to increase the bulk of the engine.

III. In point of durability and economy in its use, the most conclusive arguments lie against the rotatory engine. I have seen many of them perfectly constructed, working beautifully, but they went very soon out of order. They invariably work best when new. This may appear to some to arise only from the defects incidental to particular modes of construction. I admit that many have had peculiar elements in their construction not indispensable to the principle. But, on the other hand, I shall now go on to shew, that, independent of the idiosyncracy of peculiar engines, the necessary mode of action involves elements of self-destruction very rapid in their operation, by means of which every rotatory piston must soon wear itself out of condition.

It is a received principle in constructing machines, that, in a good engine, the parts should wear equally, and that even the very working of their parts should make them fit each other better. This is truly the case with the piston and cylinder and other appendages of the reciprocating steam-engine. So true, that old engines of Messrs Watt and Bolton—some of their earliest—are still working better than they ever did, or than some more recently made. To this the reciprocating engine necessarily presents a contrast; and it will not be difficult to shew that its parts *must* wear unequally, so as to become unfit for use, and be rendered by each day's work less fit for the duty of the succeeding one.

To shew the cause of this:—Suppose two perfectly flat plates of metal, perfectly round, to be laid one upon the other, so as exactly to coincide at every point. Let the undermost rest upon a table, and let the uppermost be so made as to turn round on an axis while in contact with the other, and let a rapid motion be communicated to the uppermost,—let me ask what will be the result of the attrition of the one of these upon the other? Will they wear equally, so as to remain in a state of mutual adaptation, or will they not? Experience furnishes an answer which exactly quadrates with reasonable expectation;—they will *not* wear equally,—they will *not* retain their forms. Let it be considered that the outer edge performs a larger circuit than any part nearer the centre; that, therefore, as all the parts revolve in the same time, those nearer the circumference move with a greater velocity than those towards the centre; that the attrition is most rapid at the circumference, and uniformly diminishes towards the centre of the plates; then it inevitably follows that *the plates must become conical*, with a continual tendency to become more so. This is a most incontestible truth. It is one which has caused the failure of many beautiful inventions; it is the reason why conical bearings have been universally abandoned for cylindrical ones; and it is the cause that has rendered a most beautiful class of inventions totally useless to the improvement of the reciprocating engine. I allude to the flat *revolving valves* introduced by Oliver Evans, and afterwards brought into this country, but now universally abandoned, in spite of simplicity, efficiency, and economy, on account of this very attrition from a centre, which we consider to be ruinous to every steam-engine on a revolving principle.

The application of the result of this illustrative experiment to the subject in question, is abundantly obvious, the circumstance of rotation from a centre, with pressure on bearing surfaces of which the parts are at unequal distances from the centre, implies the excessive attrition of the circumferential surfaces above those which are near the centre, and which move with less velocity. Hence the circumferential surfaces wear more rapidly, and are unfit for use long before the central parts have suffered any sensible effect. Where extensive metallic surfaces are in contact, their repair is a matter of much expense and delay.

To diminish this cause, or to delay its effect, the revolving piston is removed far from the centre of action. By this means, however, the bulk of the machine, and its friction, are very much increased, and the evil only partially remedied. It is obvious, however, that in this way, by increasing the radius, the engine is brought more nearly to the principle of the straight cylinder; so that perfection would just be attained if the circular cylinder were made straight, or, in other words, if the rotatory engine were converted into an ordinary reciprocating engine.

When a piston reciprocates in a straight cylinder, all its points, and those of the cylinder, move equally, being in lines parallel to the axis; and to prevent accumulation of eccentricity, the piston may have its position on the circumference altered by part of a turn.

The essential nature of *rotatory attrition* is therefore fatal to the success of the revolving principle,—a cause of expensive repairs, and speedy destruction.

IV. There are other defects to which this species of engine is peculiarly liable;—to vacuities and losses at the valves and passages—to irregular action, and collisions and shocks from the action of the parts upon one another; but these will be the subject of consideration as they occur in individual machines.

V. Unless, therefore, we shall find that there is some peculiar applicability in this form of engine above the common one, to certain important purposes, such as steam navigation and inland transport, we must abandon the hope of deriving practical advantage from the engine of rotation.

Now, it has been proposed for Steam Navigation, but to this it is peculiarly inapplicable. In a steam-vessel, it is useful to have the axis of rotation as *high* and the weight of the engine as *low* as possible. Now, if the engine be placed on this axis, as it would be in the case of the rotatory engine, one of two evils would follow; the axis would either require to be much lowered, or the weight of the engine would be so high as to make the vessel the very reverse of steady. By such a disposition of its parts it must necessarily be rendered crank, and have its power greatly diminished. In the present engine, the weight is immediately above the floor of the vessel, and the axis in contact with the deck.

Applied directly to the axis of a Steam-Carriage or Locomotive Engine, there are insuperable objections to the rotatory engine. As there would be no spring between it and the wheels, every jolt would derange the machinery. The weight of the engine, rigidly connected to the axle, would reciprocate the evil, and knock the wheels to pieces. These evils are prevented in the reciprocating engine, by the detachment of the engine from the axle, and the propagation of power through rods, wheels, or chains, to the propelling wheel or axis. It is indeed a radical defect in some of the existing forms of the locomotive engine, that the detachment is less perfect than might be desired. This very adjustment, so impracticable with the rotatory engine, was, even with the superior facilities presented by the form of reciprocating locomotion, one of the greatest impediments to the success of elemental locomotion.

VI. In addition to the above-mentioned advantages possessed by the reciprocating engine above the rotatory one, it presents facilities (altogether wanting to the latter) for working directly the subordinate appendages of the steam-engine, such as cold-water pump, its own feeding pump, &c. If the engine be a condenser, the simplicity of the reciprocating mechanism of the air-pump puts the rotatory engine altogether hors de combat.

VII. All these considerations, of the most important practical bearing, demonstrate clearly to us, that if there be no very considerable loss of power in the reciprocating engine, we have little inducement to make the substitute of the rotatory chamber and revolving piston for the cylinder and reciprocating piston.

It appears, on the contrary, both from theory and the practical working of the steam-engine of ordinary construction, that, with a very small allowance for friction, the piston gives out through the crank, in actual work done, all the power of all the steam applied to it in the cylinder. Mechanism can do no more.[*] And since neither simplicity of action, compactness of form, condensation of bulk, nor economy, either in first cost or operation, give it a superiority to the common engine, but that, on the other hand, from the very nature of its movement, it possesses the elements of rapid detrition and unequal deterioration, and is, by the necessary arrangement of its parts, rendered peculiarly inapplicable to such important objects as the purposes of steam navigation and land transport, I do not see what motive can possibly remain for devoting a single thought to its further improvement, or the alteration of its form, when its very principle holds out no higher premium than that, if brought to its utmost perfection, it might possibly approach in durability and efficiency the ordinary reciprocating engine, but in no point of view could ever excel it. To expend more time and mind on such a subject, is therefore merely sowing the wind to reap the whirlwind.

VIII. The force of the exposure I have now been induced to make of the fallacious nature of those attractions by which the rotatory motion has drawn aside ingenious mechanists from the direct path of legitimate invention into the fruitless pursuit after ingenious trifles, will have considerable weight added to it, if we turn our attention to the *peculiarities of the crank* as one of the elementary machines for the conversion of reciprocating into rotatory action.

The crank, as the means of converting the reciprocation of the piston of a steam-engine into continuous rotatory action, possesses singular and beautiful properties, which distinguish it from every other means of producing that conversion, and which appear to be so perfectly adapted to the nature of steam and the constitution of solid matter, that we are indebted to it materially, although indirectly, for the very great advantages we derive from the modern steam-engine as a source of mechanical power. Ingenuity has been taxed to the utmost to find substitutes for it, which should remedy the (imaginary) defects of the crank, but the mighty element has disdained them all, pounded them to powder, and thrown them from her. Like unskilful keepers, they have attempted to control a power by means which have only encountered the force they were designed to direct; and, after many vain efforts, it is found that the crank is the magic rod under which alone the mighty force of the element becomes peaceful and docile. Wheels, sectors, and racks, in various combinations, have been made to assume the functions of the crank, but they have uniformly been declared incapable. Once or twice it has happened that a substitute was obtained, but it was soon found that these (the sun and planet motion, for example) were only the crank in disguise; and the useless mask was speedily dispensed with when the cause of its assumption had ceased to exist. It was an invidious patent alone that induced the immortal Watt to give the name of Sun and Planet to two wheels, placed one at the base and another at the apex of the crank. The disguise disappeared as the patent expired, and the simple unencumbered crank resumed its well-merited station.

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[*] We have before us the printed reports of last year, in which the duty done by the crank engines of Charlestown and Wheel Kitty constructed by Mr Sims is given. We have also before us the indications of pressure in the cylinder, as obtained by a very accurate indicator applied in the course of last summer by Mr Smith of Manchester, who visited the mines for that purpose, and has been kind enough to favour us with a copy of his observations. We have thus the means of comparing the power actually exerted on the piston with the work done, and find the result of the comparison to be, that *the work done is within ten per cent. of being perfectly equal to the power employed.* Here, then, we arrive at this conclusion, that the utmost conceivable reach of improvement in the mechanism engine, if it attained even to perfection, would not save more than a few per cents. That the crank engine therefore, as at present used, is as near the perfection of mechanism as any thing we can hope to obtain, is, we think, satisfactorily established.

The peculiarities of the crank which give it its unapproachable perfection as an elementary machine, I shall now go on to describe.

1st, I would observe, that in the reciprocating piston in a steam-engine the following things occur:—The piston is to be put in motion in one direction, then stopped; then put in motion in the opposite direction, stopped again; motion in the original direction begun and once more made to cease. At the commencement of the motion downward, a valve is to be opened for the entrance of the steam above the piston, which valve must be closed at the end of the stroke, and at the same instant in which one steam-valve closes, an opposite one must be opened to admit steam below the piston; at the same instant, also, a valve of eduction for the first portion of the steam must be opened, and a second valve of emission on the opposite side of the piston closed. At one and the same instant, therefore, the motion of the piston has to be stopped in one direction and commenced in the opposite direction, one steam communication closed, a second opened, a third of eduction cut off, and a fourth renewed and all this (for the perfection of the engine) must be done with the most absolute precision.

But these processes, which produce the change of state from rest to motion, and from motion to rest, require time. Matter acquires momentum which must be gradually removed, otherwise that matter is subjected to concussion, as if by the stroke of a hammer, and either suffers or produces injury. And, on the other hand, when in motion, matter requires a force to stop it equal to the force that gives it that motion. These effects, therefore, cannot be instantaneous;* and it is necessary that while the motion which the steam gives off be uniform and continuous, the parts of the engine itself shall be allowed time to be brought to a state of rest, without shock, concussion, or jolt, and as gradually and gently be again urged to their greatest velocity in the opposite direction. *All these with exquisite adjustment the crank effects;* it stops the piston as gently and softly as if it placed beneath it a cushion of eider down, and afterwards as gradually begins and accelerates its motion to its highest velocity in the opposite direction. The valves, too, are opened with the same perfect adjustment, being performed with that gradual motion which proportions the largeness of the aperture to the supply of fluid required to be transmitted. An adjustment so complete could only take place by such a relation as subsists between the crank and piston, the one describing uniformly the circumference of a circle, while the other moves by simultaneous gradations of alternately increasing and diminishing extent. But this is not all that distinguishes the crank.

2d, It is one of the highest recommendations of a piece of mechanism, that any very slight error in its construction shall not very materially prevent its usefulness, nor any slight derangement of its adjustment be attended with immediate destruction, but that, on the other hand, the *efficiency of the mechanism shall*

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* "On sait que pression ne peut pas produire tout-à-coup une vitesse finie." —Lagrange, Mech. Analyt., p. ii. sec. x.

be consistent with such degrees of correctness as ordinary workmen can accomplish, and with such care as ordinary attendants can be trusted to bestow; also, that the progress of disrepair shall be so gradual as to give timely warning of the necessary readjustment. Just such a piece of mechanism *is* the crank. It is at the top and bottom of the stroke, or in the line of the centres, as it is technically called, that the opening and shutting of the valves should take place; and it is just at this point that pressure on the piston can produce no effect on the crank; but suppose the valves not to open with absolute precision, suppose them to open and shut too soon or too late, then will the error at that part of the circuit be of comparatively small importance, because, *just then*, the motion of the piston is so slight, that, through an arc of twenty degrees, it does not describe one hundredth part of a stroke, and the effect of any error in that space will not affect the crank by more than one hundredth part of its amount; any *error of adjustment is therefore diminished in effect to one hundredth part* of what would be produced, were the motion of the piston to be uniform in portions corresponding to the arc of description, as would be the case in any other species of rotatory conversion.

3d, In like manner, errors arising out of construction, management, or wear, are diminished one hundred-fold by transmission through the crank. It has been to me matter of frequent astonishment, that although I have seen at the mouths of coal-pits, small mines, and quarries, mere remnants of engines, frail rusty old fragments of iron and wood, working so loose as scarcely to remain upright upon their basements, they were still working within 30 per cent. of their full power.

4th, To all these circumstances, I may add, that the constitution of the crank is one reason why an engine may be constructed of erroneous weight, and of the most unwieldy dimensions, without being thereby much injured in its working, because the crank acquires so slow a motion at the commencement and termination of the stroke; that it equally slowly communicates motion to all the parts of the machine, and in like manner receives from them the impetus which they give out in the act of being again slowly brought to rest towards the end of the stroke. The impetus, therefore, given to the reciprocating parts of the machine is *lent* not *lost*.

We have thus endeavoured to expose the nature of the fallacy under which they labour, who imagine that the present steam-engine, as derived from Watt, is a machine which "destroys" or "absorbs" a large portion of the power it is designed to transmit, and who look to the rotatory engines as a means of increasing the amount of the power given out in useful effect. That the rotatory engines, which appear day after day, are not new, we shew from the fact, that the five great classes which comprehend them all have been invented and re-invented by upwards of ninety individuals. That their inventions have been unsuccessful, is manifest from the non-existence of their machines in the daily use of ordinary manufactures. That the failures of these contrivances did not arise from defects accidental to the peculiar arrangements and contrivances of the engines, is rendered probable by the great variety of forms in which they have been re-invented, tried, and abandoned. That they have not failed from deficiencies in the workmanship and practical details, is rendered still more probable by the circumstance of finding among the names of inventors, those of the most eminent practical engineers. We have next shewn, that in theory, the crank of the steam-engine in common use, cannot, as has been supposed, be attended with a loss of power, as such loss would oppose the established doctrines of virtual velocities; it is shewn also from very simple and elementary considerations, that what appears to be lost in force, is resumed in velocity—that, in proportion as the mean force on the piston is greater than the mean force on the crank, in that proportion is the space described by the latter greater than the space described by the former. That the dynamical effect produced in a given time is exactly in the proportion of the steam expended in that given time; and thus have we arrived at the conclusion, that the common reciprocating crank steam-engine, has not the faults attributed to it in theory, and which the rotatory engines have been designed to remedy. We have next taken the practical view of the subject—in simplicity of parts the rotatory piston has no advantage over the reciprocating piston; in difficulty of construction the rotatory piston far exceeds the reciprocating engine—it is more expensive at the outset—it has more friction—it is more bulky, and less compact—it is inferior in precision and uniformity to the crank-engine—and there is a radical fault inherent in the very nature of rotatory mechanism, from which it follows that the rotatory engine can never be rendered either an economical or a durable machine. We have further shewn that, even if the rotatory engine could be made economical and durable, its very nature renders it unsuited to the great purposes of steam navigation and inland locomotion,—objects to which it has been considered peculiarly applicable. We deemed it an appropriate and instructive conclusion to our inquiry, to examine into the action of the crank, for the purpose of discovering what those remarkable qualities are which have given to the crank of the common steam-engine, its unrivalled superiority as an element for the production of circular motion, and a degree of perfection unattainable by any other mechanism. We have seen that well-constructed crank steam-engines are daily performing duty, which is within ten per cent. of the theoretical maximum of possible effect—of absolute perfection—that this practical perfection arises from the simplicity of the crank, from its wonderful adaptation to the nature and laws of matter and of circular motion in connection with rectilineal motion—from its reduction of errors either in construction, adjustment, or management, so as to work well without the absolute necessity of greater intelligence, expertness, and precision, than belongs to ordinary workmen; and from the compensating nature of the arrangements of its structure, by which it is accommodated, in a remarkable degree, to the necessary imperfections of all human mechanism.

It is my earnest desire, that this exposure may have the effect of inducing some of my ingenious countrymen to direct their exertions for the advancement of the arts and industry of Scotland, to other and more promising subjects of invention. A wide field is open to their exertions in the useful *applications* of the mechanical powers of the common steam-engine to the wants of growing civilization, and to the improvement of the condition of the human race. Let them direct their exertions to these objects, with the same industry and unity of purpose which they have already displayed in the pursuit of the fascinating fallacy of a rotatory steam-engine, and they will one day be reckoned in the glorious list of those who have been the benefactors of their kind, and the ornaments of Scotland.

On Metallurgical Phenomena as illustrative of Geology. By Professor Hausmann of Göttingen. Communicated by the Author. (Concluded from Vol. XXIII. p. 330.)

Of the metallurgic processes there is one in particular, which, in a certain point of view, permits a comparison with that of the process of oxidation which goes on under the crust of the earth, and which, in the present era of our planet, is proclaimed by the phenomena of the still active volcanos—*the process of the production of malleable iron from crude iron.* The crude iron obtained in blast furnaces from ores of iron, by the process of reduction and melting, contains iron combined with carbon, and in union with small quantities of various other metals besides (of which manganese is most abundant), of bases of earths, among which silicium is most usually found, and of metalloids, among which sulphur and phosphorus are not unfrequently encountered. In order to obtain the purest possible iron from this crude iron, it is melted down in various modes of preparation, and heated, when reduced to a state of fusion, by the operation of a bellows or a natural current of air, in such a manner, that the atmospherical air may come into the closest possible contact with the crude iron, and, by means of its oxygen, accomplish the oxidation, and, thereby, the separation of the foreign substances combined with the iron. Of these one portion, viz. the carbon, escapes in the form of gas, whereas another is converted into slag. Although the oxygen of the air is combined immediately with the substances which have a greater attraction to it than iron has, it is notwithstanding inevitable that a part of this metal, which exists in such a preponderating quantity, is at the same time oxidized and transferred to the *slags.* It is, meanwhile, just as comprehensible, that a change takes place in the relation in which the various ingredients of the crude iron is acted on by the oxygen during the continuance of the process, and that, consequently, the slags that are produced receive at first a proportionally greater share of earths, especially of silica, than at a later period, whereas they obtain always more of the black oxide of iron the further the process advances. The black oxide of iron, which is constantly accumulating in the *slags*, does not remain without reaction on the process of the purification of the iron in yielding oxygen to the carbon, by which means one part of the oxidized iron is again reduced, and combined with the rest of the iron mass. The nearer the mass approaches purity, the further is it removed from a state of fluidity; and while, by the influence of suitable manipulations, the particles of the iron are united to a mass which is more or less encompassed by the slags formed during the process, the said mass passes gradually into the state in which, by due pressure, it may be moulded into any form at pleasure.

Now, if we compare this process with the theory of the plutonic and volcanic rocks above alluded to, a strong analogy will be conspicuous. Deviations occur chiefly in this, that in the mass of the earth's nucleus, out of which those mountain masses are generated, a greater variety of ingredients must be assumed than usually exist in the ore, as well as a different quantitative relation; and that, at the commencement and during the continuance of the great process of terrestrial transformation, water indubitably plays a chief part, whereas in the refining of iron (*eisenfrisch* process) it is principally the oxygen of the air which produces the oxidation. What we have alleged above concerning the latter process makes it apparent, how, in the oxidation of the earth's actual surface, one part of the ingredients might undergo a change, while another remained protected against it. That process shews that, although the atmospherical air chiefly affects merely the surface of the crude iron mass, yet the substances that have a closer affinity to oxygen than iron has, are gradually separated from it even in the interior. Something of the same kind, too, we may be allowed to assume, in the process of oxidation that goes forward on the earth's actual surface, since even here, when the substances that have a close affinity to oxygen, combine with it, and thereby separate from the unoxydized substances, a compensation to the same substances ensues from the mass that exists beneath it. The formation of *slags* in the refining process teaches us, that from the same mass at various times, slags of very various composition may arise. Proceeding from a mass which contains several ingredients in various degrees of affinity to oxygen, the oxides, whose bases have a closer relation to oxygen, abound in the slags of earlier creation in greater quantity than in those of later creation. A similar connection is proved, as was already mentioned above, from a comparison of the plutonic and volcanic rocks. We can, therefore, form an idea of their successive origin, without being reduced to the necessity of assuming a great inequality in the distribution of the substances contained in the nucleus of the earth. But the analogy may be still farther traced; for, even in the composition of the slags that arise from the process of the production of iron, there is a striking resemblance to the nature of many volcanic rocks; a resemblance which consists not in the considerable amount of iron ingredients alone, but chiefly in a certain silicate which is peculiar to those volcanic rocks, and whose substitute is a very ordinary attendant on basaltic rocks. The slags that are formed in the early period of the refining process, usually called *rohschlacken*, consist chiefly of combinations of silica with protoxide of iron, and some other bases of smaller quantity, which have a more or less close approximation to the relations of simple silicates; and there not unfrequently appear with a fixed relative proportion of ingredients, crystallised slags, which consist chiefly of a simple silicate of the protoxide of iron, and harmonise with the chrysolite or olivine both in a stoechiometrical relation of composition, and also in regard to the system of crystallisation. With respect to their ingredients, a difference between the crystalline slags and the olivine consists in the circumstance, that, while in the former, the protoxide of iron predominates, in the latter, magnesia abounds. But it is well known that a reciprocal substitution is effected, and while in crystallised slags we not unfrequently meet with magnesia joined to the protoxide of iron, a considerable quantity of the protoxide of iron is always found in olivine. The close affinity that subsists between the olivine and crystallised slags has found a beautiful confirmation in the discovery of the hyalosiderite, which likewise is found in basaltic rocks, and which, in regard to its chemical constitution, occupies the middle place between these two bodies above named, being composed of nearly equal parts of protoxide of iron and magnesia.

After the attempt to employ metallurgic experiments in the elucidation of the igneous process that lies profoundly concealed in the bowels of the earth, it will appear less venturous, to found explanations of the formation of the products of that process on a comparison of these products that are obvious to view, with those of metallurgic processes.

Among those parts of the earth's crust, on which the subterraneous fiery process has exerted an influence, we recognise partly such productions as are indebted to that process entirely for their origin; partly, masses that have been generated by other causes, and which, either by the communication of heat or by the immediate operation of the productions of the fire, have undergone more or less considerable change. The state which immediately preceded that in which we at present perceive those productions of the fiery process, might be either that of a vapour, a liquid, or a paste. The masses that are now-a-days ejected from volcanic eruptions are found in all these various states; and we are assuredly justified in assuming that other masses whose fiery origin indeed is not to be questioned, but the nature of whose formation still remains hid, existed in one or the other of those conditions before they passed into a state of solidity: but here it is still not to be forgotten, that the state in which these masses attained their present condition was often perfectly different from that in which they originally existed. It would be of importance to geological investigations, if fixed signs could be discovered, by which we might recognise the earlier state; and perhaps it is possible, from a comparative examination of the productions of the foundry, to make progress in such a discovery.

First of all, with regard to the transition of bodies from the gaseous state into the solid, a difference takes place in this,—that either the gas passes immediately into the solid state by condensation, or not until after it had assumed the liquid state: and by the same kind of evaporation, according, for instance, to the rate of cooling, may the one or the other transition take place. Instances of this are afforded by certain processes that are employed in procuring zinc and sulphur. Sometimes it is difficult to decide, whether the one or the other mode of transition ensued: for similar kinds of aggregation, particularly crystals, may arise in both ways. But we recognise with certainty the earlier molten state in which stalactitical forms, or other distinct indications of previous fluidity, appear. On the immediate transition of the vapours into the solid state, there often arises, especially when the condensation very rapidly ensues, a loose powdery or volatile aggregation, similar to what we observe in the sediment of white arsenic in roasting ovens, or in the flowers of zinc that are formed by sublimation. If we apply these experiments to a more close investigation of the mineral bodies formed by the grand sublimation process of the earth, we must certainly not overlook the circumstance, that similar forms to those above mentioned are frequently generated in the *moist* way, entirely without the operation of fire, and that consequently, these forms can only yield explanations as to the course of formation, when a general conclusion has been come to respecting their nature.

If the inquiry into the products of furnaces makes us acquainted with a variety of bodies transformed by the heat of the melting furnaces into vapour, we shall be induced to attribute to the sublimation process of the earth a more extensive range of action, than would have otherwise perhaps been our inclination. We may conclude from the nature of the productions of the foundry, that certain bodies, with whose existence in a state of vapour we were either not at all, or with but little certainty acquainted, passed from these states into solid ones. Now, if this could take place with the heat of the melting furnaces, how much greater is the possibility of the formation of vapours, by the operation of the fire of the subterraneous forge of the globe.

In the blast furnaces crude iron penetrates not unfrequently into the fissures of the mass of which the building consists: a phenomenon of little importance. But iron also is sometimes found in the interior of a sandstone which has been exposed to a high temperature, partly in veins, partly disseminated, without the slightest trace of any fissure,—so that this supposition alone appears admissible, that it penetrated in a state of vapour into the sandstone when softened by a high temperature. This kind of iron is distinguished by a very light silver-white colour, and has here and there externally a steel-grey tint, has a granular foliated structure, and is so ductile that, when even in a cold state, it may be hammered out into thin laminæ. It contains silicium and carbon, and it is worthy of remark, that silica is sometimes associated with the iron; at all events, the appearance we have described bears a striking resemblance to the manner in which certain metals and ores, especially gold, silver, and copper, are found disseminated in veins, and at the same time, in adjoining rocks; as, for instance, the native silver at Konigsberg in Norway.

With respect to the relations in which silica is found in the walls of extinguished blast-furnaces, Mr Koch has given an extremely minute account.* Its appearance is in many instances only explicable on the assumption of a penetration in the form of vapour. The above-mentioned fact of the accompaniment of the iron that is found in the interior of the stones of the building, coupled with the mineralogical association with titanium and graphite,'hereafter to be referred to, are greatly in favour of this view. Mr Koch has already drawn our attention to the impropriety of doubting that the silica proceeded from silicium, whose reduction from iron-ores rich in that substance, ensued in the highest temperature of the blast-furnace. The silicium was partly combined with the iron; and this with the crude iron as well as with the refined iron which is sometimes generated, so that it was thus protected against oxidation. But as a vapour too, it penetrated, partly with iron and carbon, also in the form of vapour, into the interior of the mass of the building, and passed here either immediately by means of oxidation into silica, or not until after having again resumed the molten condition: and this is sufficiently proved by the globular and kidney-shaped external forms, by the concentric concretionary layers, and by the arrangement of their fibres.

The well-known appearance of crystals of titanium in the products of the blast-furnace, belongs to the most interesting phenomena which these afford. The relations in which cubes of titanium are found in excavations of masses of slag, crude iron, and refined iron, sometimes accompanied by silica, partly in the building, partly even beneath it, are of such a nature as leave no room to doubt of its sublimation. The tendency of the vapours to penetrate downwards, is to be explained by the pressure of the molten mass that exists in the base.

In the case of the graphite formation also, which accompanies the production of the grey crude iron, the previous existence in the state of vapour may also be fairly assumed; for the appearance of that crystalline body, which, according to Karsten's researches, we are authorised to consider as pure carbon, is displayed in such a way, that we cannot suppose it to exist in another state immediately previous to the crystalline one. It is well known that carbon occurs in various ways in the grey crude iron, namely, partly in chemical combination with the iron, partly as graphite mixed with it. That the quantity of carbon which is contained in the liquid crude iron and which is not able to combine the iron chemically, in the moment of rigidity, and even partly while the iron is still fluid, is dissipated in vapour, and passes from this state immediately into the crystalline, appears susceptible of proof from the circumstance that the graphite is observed not only in the interior, but even on the actual surface of the crude iron, like crystals of snow on ice, formed by the ascending vapour,—that its crystals are met with on the slags, which cover the iron in the furnace, and even in their internal cavities,—and that the graphite is sometimes found even in the interior of the wall, and not unfrequently associated with those bodies (such as silica), whose penetration in a state of vapour we are compelled likewise to assume. The more slowly the cooling proceeds, the more unconfined the space, and the smaller the external pressure, the more distinct is the crystalline structure of the graphite. The largest crystals are found in the cavities of the crude iron on its actual surface, and particularly on the slags. The more slowly the crude iron cools, the more distinct appear its scales; whereas, in cases of rapid cooling, for instance, when crude iron is plunged in water, the scales are indistinct. In grey crude iron the graphite is most usually more or less uniformly mixed with the iron: in many varieties, however, among others in the Swedish and Norwegian crude iron, the mixture is not uniform; in which case, the graphite sometimes forms the completion of globular spaces, and then its crystals are grouped concentrically, as we observe in amygdaloids, resembling, for instance, the appearance of the scaly chlorites. This phenomenon might confirm the assumption, that graphite vapour formed single bubbles in crude iron; and this leads further to the supposition, that the cavities in rigid crude iron in which graphite crystals are often found, are partially generated from the carbon vapour after it has effected its escape. Particular notice is due to the appearance of graphite in the cavities of a slag of a blast-furnace at the same time with iron, which in a botryoidal form, and partly with an oxidized surface, forms the basis on which the graphite is deposited. Here we perceive manifestly that the graphite vapour was condensed after the liquid iron had been deposited in that form, on the smooth surface of the rigid slag. There is a circumstance worthy of note attending it,—iron and graphite are found always together, and only on the upper part of the hardened slag: hence we must conclude, that the iron too gained access to the slags in the form of vapour, but became liquid before it passed into a solid state; whereas, graphite was immediately crystallized. If this opinion be correct, it follows necessarily that iron requires a higher temperature for its sublimation than carbon.

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* Beyträge zur Kenntniss Crystallinischer Hüttenproducte, S. 34-40.

In the case of the phenomena described here, a comparison with the filling up of cavities in amygdaloids is forced upon us. Here too, sometimes the whole space, sometimes only part of it, appears filled, and often only the covering sprinkled. In these cases too, a certain succession is observed in the deposition of the different mineral bodies which coat or fill the cavities; here also we perceive bodies which were liquid before their transition into the solid state, and afterwards assumed either botryoidal and stalactitic, or crystalline forms: while, on the contrary, other bodies appear to have passed at once from a state of vapour into the crystalline condition. No contradiction is felt between this kind of completion of the cavities in many amygdaloids and that which takes place by filtration, and which in other cases displays itself unequivocally. The formation of a large proportion of the veins, particularly of those containing ore, bears a strong analogy to the filling of the cavities in amygdaloids; and, indeed, there not unfrequently takes place a real transition between imbedded globular and amygdaloidal masses, and the materials forming veins. The manner of alternation among various vein stones parallel to the chief bounding surfaces;—the certain regularity of that alternation, which appears, as well in the filling up, as in single cavities; the mode in which one mineral forms a layer over another; —the growth on those parts of crystals inclined downwards; and many other phenomena, are only to be satisfactorily explained, on the supposition that the mineral substances gained access to the veins in a state of vapour. The various forms which vein stones have assumed, may also be explained from the results of the condensation of vapours. Amorphous and crystalline structures might proceed either immediately from the vapours by means of their cooling, or after having first passed into the fluid state: stalactitic forms only in the latter way. We have shewn on a former occasion (Specimen Crystallographiæ Metallurgicæ,) how the imitative figures, the capillary, filiform, and dendritic forms, which occur in veins, and particularly in certain native metals, such as gold, silver, and copper, have received a satisfactory explanation from a product of the furnace, called hair-copper.

Not only is a general analogy found to subsist between certain productions of the furnace that have originated in vapours, and the formation of many veins of ore, but there is sometimes such a perfect similitude displayed, that we may be misled to confound pieces ejected from the furnace with fragments of the veins of ore. In confirmation of this statement, witness pieces found in the melting-hearth, and on the ground-stones of extinguished furnaces in the Upper Harz silver mines, which, in the mass transformed by the heat, contain veins of regenerated lead-glance, of from scarcely a measureable size to the thickness of several inches, together with ramifications, intersections, displacements, and other phenomena peculiar to veins of ore.

The phenomena exhibited by the formation of vapours, as observed among the products of the foundry, point out the essential difference that exists between veins formed by sublimation, and vein-like fillings up of fissures by masses which penetrated into them in a state of fusion. Metallic veins are of a perfectly different kind from veins of granite, porphyry, greenstone, and basalt. But even in regard to the structure of the vein in the former kind, a consideration of the productions of the foundry is indicative of a difference, which consists in this, that vapours either withdrew into already existent spaces or clefts, that had been perhaps more or less widened by their operation, or they penetrated as a loose or soft mass. Vapours penetrate in great variety into crevices in the walls of mines, or in the hearths of furnaces, and form, by condensation, partly upfillings, partly coatings, presenting themselves very often in crystals. To this class belong the most famed crystallizations of oxide of zinc from the iron blast-furnace, which are sometimes accompanied by cubes of chloride of potassium. To this also must be referred the occurrence of lead-glance, zinc-blende, and arsenic acid, which like veins fill up crevices in the walls of silver, lead, and copper-furnaces. That in certain circumstances, silicates too may be formed in a similar way, is proved by the appearance of crystals of a substance resembling felspar, derived from fragments in the copper-furnaces at Sangerhausen, to which attention has again been recently directed; but which were described by me in the year 1810, in the 4th number of the Beiträge zur Berg-und Hüttenkunde, p. 86. The same bodies which are found in this way, like some others formerly mentioned, penetrated as vapours into masses, which either, like bricks, for instance, have a certain porosity, or like sandstones and others, have been transformed by heat into a softened condition; and are in this state deposited in forms, which resemble at one time disseminated portions, at another nests of minerals, and at a third, ramified veins. This phenomenon may, in an especial manner, serve to explain the formation of many incoherent nest-like veins, which either contain ore, or consist merely of what are termed vein-stones.

The metallurgic processes afford many opportunities for the study of fused masses, and the solid bodies that are produced from them; and since the rocks of igneous origin are composed in a great measure of silicates, a more accurate examination of the slags, which consist very frequently of silicates, is peculiarly calculated to yield some clew to their formation. Among these silicates, as Mitscherlich was the first to shew, the appearance of several, which, both in texture and external relations, harmonizes with mineral substances that are of great importance in the composition of the massive mountain rocks, has contributed greatly to establish more firmly the theory that assigns to them an ignéous origin, and to procure for it more general support. But apart from the analogy that lies in their structure, the knowledge of the formation of these mountain-rocks may be promoted also by the study of the state of aggregation of slags.

When silicates pass from a fused into a solid state, either a glassy or a more or less crystalline body is generated, as the cooling is rapid or slow. These two kinds of aggregation are directly opposed to each other. In the glass, the structure is still the same as it existed in the fused body: in consequence of the rapid transition into the solid state, no time was allowed to the ingredients to combine in intimate relations: consequently, the accompanying power of crystallization likewise did not come into operation, but the whole followed the laws of common attraction. On the contrary, if the cooling is slow, the chemical attraction which arranges the ingredients in fixed relative proportions, and the power of crystallization closely associated with it, come into activity at once, and *that* in a degree of perfection proportioned to the slowness of the cooling. According to the difference of texture, there is then formed either one crystalline substance only, or there arise different substances which are chemically and crystallographically distinct. If the texture is not of such a nature, that the whole body is formed into a single or compound crystalline mass, either one crystalline body, or it may be several of them, are separated in the glassy mass. The stony state, in which the crystalline formation is confused and indistinct, occupies a middle place between the glassy state and the plainly crystalline aggregation; in consequence of which a mass is formed as a basis, which contains more perfectly crystalline bodies.

These different modifications of the state of aggregation of silicates are displayed in great variety in the slags that result from various foundry processes. Now, it must not seem surprising that perfectly fused pure slags appear usually glassy, since they most frequently quickly become solid, either from communication with the air, or from falling upon cold humid ground. But as soon as circumstances occur that are favourable to a more gradual transition from the fused to the solid state, a crystalline formation conformable to the structure of the slags takes place; and by this means a great difference in the tendency of various slags to crystallize is made apparent. Porphyritic separation of single crystalline particles, which are not unfrequently spherical, and of perfectly formed crystals, is often met with in various kinds of blast-furnace slags. The *Kieselschmelz* as described by Mr Koch, is found beneath the blast-furnace slags in the iron-foundries on the Harz, in its regular six-sided prisms, resembling crystals of the Nepheline imbedded in a glassy basis of porphyry; and something exactly similar is displayed in the iron-foundries of other parts by slags crystallized in the form of idocrase. To the most remarkable productions of this kind belongs the avanturine, a sort of glass coloured by means of protoxide of copper, and oxide of antimony, in which extremely delicate regular six-sided tables of copper-mica (a combination of three parts of the protoxide of copper, and of one part of the oxide of antimony) lie imbedded; a structure that is sometimes visible on the slags produced by the fusion of copper. These phenomena display something perfectly analogous to the occurrence of felspar crystals in the obsidian lava. This supplies us at the same time with a proof for the accuracy of that opinion, which teaches us, that most crystals found in the different kinds of lava are not formed independent of it, and merely included in its mass; but that they are produced by being separated from the lava upon its becoming solid.

The stony character of the slags occurs much more frequently than the separation of individual crystals; a circumstance which occasions the disappearance both of transparency and of glassy lustre, and leaves at the most a faint shining lustre on the surface of the fragments, which, instead of being perfectly conchoidal, occur generally with an imperfect conchoidal, an uneven, or a splintery fracture, and in consequence of the last mentioned quality, often approximate more to the crystalline radiated structure. Such slags may be compared with many compact intimately united mountain rocks, as for example, the aphanite and compact basalt, and also with the basis of many kinds of porphyry, such as eurite, clinkstone, pitchstone, and pearlstone porphyries. The slowly cooled *Leisten* and *Gossen* slags of the blast-furnace are of a stony nature; and this quality is not unfrequently found in the interior of a larger mass of slags with an externally glassy texture. The stripping of the glass from the blast-furnace slags, which is effected by a slow cooling among the rubbish between gradually extinguishing small coke, as is practised in the Royal Foundry in Silesia, is very curious. The slags so stripped, have often a great resemblance to porcelain jasper, and are usually, externally, of a grey, and internally, of a dark-blue colour.

Of all the various kinds of slags there is none that appears more frequently crystallized than the silicate of the protoxide of iron above alluded to, which, as was first shewn in the *Specimen Crystallographiæ Metallurgicæ*, is produced not only by processes employed in the manufacture of malleable iron and steel, but by those also of copper-foundries. Among the blast-furnace slags, too, are sometimes found such as are wholly crystalline, partly foliated, partly radiated. Of this description is the *Kieselschmelz* slags having the stoechiometrical relations of pyroxene, and bearing an extraordinary resemblance to many kinds of diopside in its external characters; and likewise a kind of slag which consists chiefly of a bisilicate of lime, and consequently resembles the Wollastonite in its composition.

The slags most rarely found, are those which are analogous to the rocks composed of various crystalline minerals, in which the ingredients of the fused entire mass have been separated according to the fixed relative proportions on its becoming solid, and by which the whole has become a union of different determinate combinations. A formation of this kind is sometimes presented by blast-furnace slags, in which crystals of *kieselschmelz* lie imbedded, in a foliated-radiated mass of pyroxene slags, so that the whole bears some resemblance to the nepheline dolerite, as described by Von Leonhard. In the crystalline slags cavities are frequently found, in which crystals are more or less perfectly formed, wherein the most perfect analogy is displayed with drusy cavities in granite and other mountain rocks composed of silicates. Now, if we compare in general the phenomena observed on the slags composed of silicates with analogous massive rocks, it must appear remarkable, that among the plutonic mountain rocks, the crystalline structure preponderates; for we find the granular varieties by far the most abundant, the porphyritic and compact being much less frequent, and the glassy scarcely found at all; whereas in the volcanic masses the perfect crystalline structure is of rare occurrence; the porphyritic as well as the compact appear on the whole more generally, and even the glassy kind is not unfrequently met with. To this must be added another difference, that a greater uniformity of internal structure belongs to the plutonic than to the volcanic rocks. Here again, therefore, there is displayed a much greater analogy between the products of our melting furnaces and more recent products of the great subterraneous fire, than exists between the former and earlier productions of the latter, a circumstance which it is easy to account for by observations made on the structure of slags. The plutonic rocks are manifestly formed by a very slow cooling; and since their masses are partly of very large dimensions, the cooling might take on the whole a more uniform course, and consequently produce greater uniformity in the internal structure. Towards their boundaries the plutonic masses display frequently the influence of a more rapid cooling. Hence granite and syenite on their boundaries have often a porphyritic character; and the large-grained granite has sometimes a coating which is fine grained, and passes into hornstone. The volcanic rocks underwent, on the contrary, a more rapid cooling, and that, indeed, in proportion to the recency of their formation; their masses, too, have usually a much smaller compass. Hence, in regard to them, we often observe in inconsiderable spaces a remarkable variety of internal structure; hence, for instance, in vein-like masses of dolerite the interior is often crystalline, whereas the external part appears compact.

An additional circumstance appears to have exerted special influence on the predominant crystalline formation of the plutonic masses, viz. the prevalence of silica. The formation of several silicates, which are distinguished for their great tendency to crystallize, as well as that of mica and felspar, was thereby effected, and in case of an excess of silica, the entire mass might pass into crystalline combinations, the free silica having become separated in the form of quartz. With a diminution of the quantity of silica, came also into play, in the case of the plutonic rocks, a diminution of their tendency to chemical individuality and crystalline structure, as is observed in the hornblendic and pyroxenic rocks. The same consequence undoubtedly ensued in the case of a very large superabundance of silica, whereby the formation of eurite and hornstone porphyry was chiefly occasioned.

We shall now once more revert to the most usual kind of slags, viz. the glassy, in order to attain a more accurate explanation in regard to certain phenomena in volcanic masses. The form and structure of these masses, as well as of the glassy lava, are entirely accommodated to the laws of common attraction, and are consequently essentially different from that which appertains to crystallization. The chief form of the amorphous masses is the globular. In favourable circumstances this form appears very distinct in glassy slags. It displays itself on the free surface of blast-furnace slags, and is found likewise in every other kind of slags, for instance, in the melting processes of lead and copper. In like manner obsidian takes a globular shape, whether it be confined in small quantities like marekanite in pearlstone, or form, as in Iceland, whole streams of lava.

The most regular form assumed by amorphous masses is that of hexagonal prisms. We conceive their formation to resemble the mutual flattening of equally large globes, placed in juxtaposition, occupying mutually such a station, that lines drawn through their centre form equilateral triangles. This structure, which every glass can assume, but which, however, is often found in such substances not glassy, as have passed from the fused into the solid state, is sometimes found in perfection in slags. We may here and there indeed trace on their actual surface the original transition from the globular formation to the perfect hexagonal prism, and at the same time understand how irregular prisms arise from a difference of magnitude in the adjoining spheres of attraction ; the same phenomena, which in certain circumstances are displayed on a grand scale in torrents of lava in common basalt, and in various other ignigenous rocks.

The formation of vesicular cavities in the slags merits particular consideration, because, in regard to this, the most perfect analogy is displayed with what is perceived in many volcanic rocks, and especially in streams of lava. The vesicular structure has in both cases a twofold origin ; either resulting from vapours and gases which, during the formation of the fused mass, were received by it ; or produced by a formation of vapour which is to be ascribed to an accidental approximation of moisture. The formation of cavities of the former kind has been already treated of, in the description of the appearance of iron and graphite in the vesicular spaces of blast-furnace slags. Where the latter cause exists, both slags and torrents of lava display the greatest number of cavities in places where they come in contact with a moist surface, and on the actual surface on which moisture was operating in one way or another. In the case of glassy slags, if water be poured upon the surface of a vesicular mass, it passes into a porous mass, which has sometimes a striking resemblance to pumice that is formed on the surface of obsidian lava, when it comes in contact with water, as for instance, with the water of the sea. As the vesicular cavities of slags and lava, guided by the motion of a more or less coagulated mass, are at the same time sometimes flattened by pressure, and from the position of the longer axis of their figure approximate the elliptic form, even in a state of solidity, continue to point out the direction of the torrent; in like manner, from the elongation of single particles of the glassy slags, as well as of pumice, arises the thread-like structure, which must not be confounded with the *fibrous* modification of crystalline separation. Another phenomenon appertaining to this class, of similar origin to the spun glass, is found in glassy slags, viz. a loose complication of the most delicate glass threads. It is sometimes found in the frames of blast-furnaces when the wind strikes it, and from the opposite current of air which is produced in this way, many small globular slags are thrust into this shape and elongated. A similar phenomenon is sometimes produced in volcanic eruptions; on the island of Bourbon, for instance, in the year 1821, there was observed a shower of ashes consisting of the finest glass threads.

We may now assume, as was formerly observed, that the igneous process of the earth has exerted a varying influence upon mountains also of a different origin. In this respect, too, a more intimate study of the productions of the foundry may prove instructive. Apart from the influence which the development of vapours, and the ascent of fused masses, must have on the position of those masses which form the vault of the grand subterraneous furnace, partly the heat, and partly the heterogeneous substances might have produced changes in them. The metallurgic processes give rise to phenomena which bear a striking resemblance to these substances.

To the most remarkable changes which the heat of the melting furnace produces in bodies which are affected by it, belongs unquestionably the softening of the hard grey limestone alluded to on other occasions (Gött. Gel. Anz. von 1816, p. 494), which is used as a building stone for blast-furnaces of some mining districts in Sweden, without losing its carbonic acid, or undergoing any other perceptible changes. Many phenomena displayed by the changes that have ensued in the position of the limestone strata, would not be explicable could we not assume their softening by means of heat; an assumption greatly favoured by our experience. If in blast-furnaces the pressure of the fluid masses effects the conservation of carbonic acid in limestone, the same consequence ensued to the lime strata from the great pressure of the sea.

The heat of the melting furnace often produces in stones and other masses contained in their centre, lasting changes, of a perfect analogy to those displayed in mountain rocks, which, by some means or other, have come in contact with masses to which an igneous origin is ascribed. Clay-slate, which was used for constructing a blast-furnace at Mägdesprung, in the Anhalt territory, had, from long operation of the heat, without having become fluid, acquired an aspect like that of siliceous slate, an appearance very much resembling the rock which is often found in the neighbourhood of diabase, where this rock is in contact with clay-slate. The changes have already been described (Göt. Gel. Anz. v. 1816, p. 490–493) which sandstones in the wall of the pit or of the melting furnaces undergo from the heat. The rocks pass into a more or less altered mass, by which the grains disappear the more the farther the heating effect progresses. By a smaller degree of the process the fracture appears dull; by a higher degree it assumes some lustre. The rocks become at the same time rough and sonorous, often quite resembling the quartz masses which are found in many districts; for instance, near Cassel and Dransfield, in the neighbourhood of basalt. The sandstone usually loses the yellow or red colour, with which it is originally invested; a process apparently effected by the transformation of the hydrate of the peroxide of iron, or the peroxide into the black oxide. In those places where the oxide of iron was more accumulated in the beds of clay or marl that are found in sandstone, there have arisen dark streaks or stripes of a heated appearance, in consequence of a greater concentration of the black oxide of iron. Besides these changes, there is not unfrequently displayed in the building stones a division into prisms, which we have already mentioned. This entire series of phenomena is sometimes visible in strata of sandstone, in spots where they are penetrated by a basaltic mass, as happens, for instance, on the Blaue Kuppe, near Eschwege; on the Pflasterkaute, not far from Eisenach; and at Wildenstein, near Büdingen. The change in the sandstone seldom progresses so far as to form a crystalline structure, as was found by me to take place in stones procured from the wall of an extinguished blast furnace on the Harz. The change in the sandstone may be traced from the point where it possesses a granular structure, and a yellow tint, to where it becomes a completely compact altered grey mass; and thence into a cellular crystalline mass, whose cavities are furnished with small crystalline tables of a pearl-grey colour, with a pearly lustre, and a distinct cleavage, and which, before the blowpipe, may easily be melted to glass, accompanied by some effervescence. The crystalline laminæ have a resemblance to mica. This phenomenon is, in a geological respect, of deep interest, inasmuch as it seems greatly to favour the conjecture that has been started in modern times, of the possibility of crystalline rocks being produced from conglomerates and sandstones under the influence of heat. Whether sandstones alone have contributed materials to effect that transformation, or potash perhaps from the ashes of coal been superadded, by which the fusibility was augmented, and the formation of a crystalline silicate promoted; at all events, this phenomenon may be considered analogous to the process that is found in many conglomerates, and in many sandstones, where the heat of masses that have ascended in a fused state has had influence upon them.

It was incidentally observed in the former part of this treatise, that bodies which have been transformed into vapours by the heat of melting furnaces, sometimes penetrate in this form into stones and other masses with which they come in contact. In consequence of this circumstance, the qualities of the masses are sometimes completely changed. The following facts deserve to be mentioned, as illustrations of this kind of transformation:—The bases of the melting furnaces in the Upper Harz silver-mines, consisting of variegated sandstone, are sometimes entirely penetrated with vapours of the oxide of lead, which communicates to the sandstone a lemon-yellow colour. The quartz grains may still be partly distinguished; a silicate of the oxide of lead, however, has partly been formed, which appears here and there fused and vesicular. Sandstone from the lowest stratum of the masonry over the frame of the blast furnace that was extinguished in the year 1823 at Elend in the Harz, had not only received a black, speckled appearance from the penetration of graphite in vapour, that had deposited itself in the form of powder, but even the originally solid mass was converted into thin undulating, easily separable beds. Such observations render it highly probable that whole mountain masses have here and there gained a different structure from the penetration of vapours. Is it not probable that the oxide of iron, which penetrates mountain masses in which layers of red ironstone are imbedded, may have gained access to them in the form of vapour? and, May not the occurrence of the graphite which sometimes impregnates whole mountain strata, and is found concentrated in particular masses, be explicable by the circumstance just mentioned?

Certain metallurgic productions have likewise elucidated the fact, that fused mountain masses which have forced their way through others of an earlier creation might change later ones, not only by the heat which they communicated, but even by penetration. The penetration of litharge into the hearth of the refining oven is sufficiently known, and presents nothing striking, the mass being porous. We observe an occurrence exactly analogous to this in a stream of lava, which, as it sweeps along a loose surface, penetrates into the earth. More worthy of note is the phenomenon which I remarked on pieces of the forepart of the building of an extinguished blast-furnace at the Steinrennerhütte, in the Harz. Veins of a silicate of the black oxide of iron, resembling slags of the refining process, cross in different directions the sandstone changed by the heat. The veins which ramify towards the sides are of different sizes, extending perhaps to half an inch, and have evidently ensued from the penetration of fused masses into the fissures of the sandstone. Here and there are vesicular cavities which appear partly lined with crystals of that silicate. The sandstone is, on the whole, loose, but becomes gradually more solid and altered as it approaches the veins. The immediate boundary of the vein forms a solid mass of an altered appearance, and of a greenish-grey colour, varying in extent from the width of a few lines to the size of several inches, and completely transformed by the penetration of the silicate of the protoxide of iron; in which mass too, the individual quartz grains of the sandstone may still be distinguished. The transformed sandstone is partly distinctly separated from the mass of the vein and partly amalgamated with it. These relations display a striking analogy with the phenomena, which accompany granite in several places, where, as takes place in the Harz, it is in contact with the greywacke. The chert which surrounds the granite, and which is often just as intimately associated with it as with the greywacke, appears to have been formed by the penetration of the mass of granite into the adjoining mountain rock. The most perfect amalgamation often takes place, but still the individual ingredients also are often visible, and distinct granite is found branching away into the intimately blended mass. In the same proportion in which the crystalline structure of the granite disappears, the conglomerated nature of the greywacke comes more prominently into view.

This parallel induces us, at the close of this inquiry, to direct the attention to still one particular, which has reference to the whole subject. The inconsistency, which a comparison of such diminutive phenomena as those of the processes of the foundry, with the gigantic organization of the earth's crust, might have for many individuals, disappears, as soon as we distinctly perceive, that it is not *absolute*, but merely *relative* greatness, that comes into consideration. A vein of lead-glance, a few lines in breadth, found in the wall of a melting furnace, has proportionally much larger dimensions, than the most stupendous veins of ore in our mountains: and if a piece of sandstone, a cubic foot in contents, be traversed by a fused body half an inch in size, which has, by penetration, caused the removal of the contiguous stone to the distance, it may be, of a few lines, or of several inches, the change effected *here* is of much greater importance than that which is perceived in the influence exerted by the granite of our Harz on the adjoining greywacke. If, then, in a melting furnace such effects could be produced by vapours, or in the filling up of fissures by means of fused masses, how much greater is the probability that similar phenomena might ensue from the ascent of vapours and fused masses from the grand furnace of the earth? And here we may call to mind the sentiment of Seneca, which those who engage in geological researches should bear continually in remembrance: "Magna ista, quia parvi sumus credimus. Multis rebus non ex natura sua, sed ex humilitate nostra, magnitudo est."

Memoir concerning the Effect which the clearing of Land has in diminishing the Quantity of Water in the Streams of the District. By M. Boussingault.

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Whether the labours of the agriculturist are effecting any modification in the climates of the countries subjected to their agency, is an interesting question which is now very generally canvassed. It is moreover inquired, whether the immense clearing away of woods in some districts, together with the draining of morasses, which exert so much sway over the distribution of heat during the various seasons of the year, exert likewise an influence upon the streams and rivers which water the district, either by diminishing the quantity of water which falls, or by inducing a more rapid evaporation from the surface, when the wide spreading forest has been laid low, and its place been supplied by well cultivated fields.

A variety of observations would indicate that such a change has been, and is being, effected. In many localities it has been thought manifest, that, for a certain number of years, the streams which were employed in propelling water-wheels have very sensibly diminished. In other places, many have been led to conclude that the rivers have become more shallow; and the increasing extent of their beds, covered with pebbles on either side, appear to attest the disappearance of a portion of their waters. Finally, many abundant springs are now almost dry. These observations have principally been made in valleys which are surrounded by mountains; and it has been thought apparent, that this diminution in the quantity of the waters, has been nearly coincident with the epoch when the hatchet was employed, without mercy or judgment, against the woods which were widely distributed over the surface of the country.

These facts seem to indicate that in those regions where the process of clearing has been extensively carried forward, less rain falls than formerly. This, in truth, is the opinion which most generally prevails upon the point; and if admitted without further examination, we must necessarily yield to the conclusion that the clearing away of the forests diminishes the annual quantity of the rain which falls upon the district. But on the other hand, even allowing that all the circumstances alluded to have been satisfactorily ascertained, still it has also been observed, that since the clearing of the mountains, the different torrents and rivers which seem to have lost a portion of their waters, occasionally manifest such sudden and extraordinary rises, that extensive devastations are the frequent consequence. It has likewise been observed, that after great falls of rain the springs which had almost entirely disappeared, suddenly rise with unusual impetuosity, only to subside with corresponding rapidity. The natural inference from these latter observations, as will be at once perceived, is, that we are not too readily to adopt the common opinion, and to admit that the cutting down of the woods diminishes the annual fall of rain: for it may not be at all impossible, not only that the actual quantity of rain has not varied, but that the quantity of water passing off in the running streams may be really the same, in spite of the apparent drought at certain periods of the year, both in the rivers and the springs; and possibly the only difference will turn out to be, that the flowing of the same mass of water has, owing to the clearing, become much more irregular. In illustration, we may remark, that if the small quantity of water which is found in the Rhone, during a certain part of the year, was precisely compensated by a sufficient number of great floods, the necessary consequence would be, that it still conveys to the Mediterranean the same volume of water which it did, at the epoch anterior to the extensive clearings which have been effected near its principal sources, and when, probably, its mean depth was not, as it now is, subject to great variations. If this were actually the case, the existence of the forests would be attended with this advantage, that they would in some degree regulate and equalize the flow of the water. But if, on the other hand, the whole annual quantity of the flowing water becomes less as the clearing away of the forests extends, then the effect must be attributed either to the rains becoming less abundant, or to its evaporation being promoted by the ground not being covered with trees, and being thus deprived of shelter against the sun's rays, and the wind. These two causes, which always produce a similar effect, must generally be combined. Before, however, endeavouring to estimate their influence separately, it will be proper satisfactorily to ascertain if it be irrefragably established that the water-courses of a country where clearing has been extensively accomplished, are truly diminished; and thus assure ourselves, that the mere appearance of the phenomenon be not taken for its reality. And after all, this is the important point of the inquiry; for let it be once established, that the clearing of a country reduces the quantity of water in its running streams, the cause of this diminution is of secondary importance. We must now, therefore, inquire if we can find in nature a set of phenomena which will act as a criterion in resolving this question.

I regard lakes, whether met with in plains, or in different stages of mountain chains, as highly qualified to throw light upon this discussion. These may be considered as natural gauges, calculated to assist in valuing, upon a great scale, the variations which may take place in the quantities of water which fertilize a country. If the volume of these waters undergo any variation, more or less, it is manifest that this variation, whether of excess or diminution, will be indicated by the mean level of a lake, inasmuch as its mean level varies at different times of the year, according as the season is wet or dry. Thus, the mean level of a lake will fall, if the annual quantity of running water in the streams of the district diminishes; on the contrary, it will rise, if these streams become more copious, and, finally, the level will remain stationary, if the volume of water which runs into the lake experiences no change. In the following discussion, I have preferred the use of those observations only which relate to lakes which have no outlet; and the reason will be apparent, as we wish to ascertain changes, which may not be very considerable. However, I do not disregard such lakes as have an issue for their waters; for I am certain that the study of such will also lead to very precise results. Before proceeding farther, I shall say a few words on the meaning I attach to the phrase, *change of level.*

Geologists have observed, that in every region of the globe the level of the waters has undergone marked variations, whether as it respects the coasts of the ocean, or the margin of great lakes. The fact is certain, and is now questioned by no one. There is not, however, the same unanimity as to the cause of the phenomenon. Some, and these the greater number, maintain that in most cases the change of level is only apparent, and that the masses of water have in no degree sunk, but that the shores have been uplifted. Others, again, recognise in the phenomenon a real diminution of the liquid mass,—a true drying up. Both of these parties adduce various reasons in support of their several hypotheses. On this occasion, however, we are not called to enter upon this geological disputation. We shall have nothing to do with the sea-coasts; nor shall we any more regard those great differences in the level which have evidently occurred in some lakes, as the result of certain geological causes which are quite foreign to our present subject. These variations, which are often very great, appear in general to have been occasioned by violent catastrophes, which have occurred anterior to the era of history. I shall make use then, only of those changes in the levels of lakes which have been observed by our ancestors, or our contemporaries; or, in other words, I shall regard as valuable those events only which have happened under the inspection of mankind, and this, because it is the effect of their agricultural labours upon the meteorological state of the atmosphere, that I wish to appreciate. America has been the principal site of the observations which follow; whilst at the same time it will appear that what is true respecting the New world, is no less applicable to the Old.

It will not be disputed that one of the most interesting districts of the kingdom of Venezuela is the valley of Aragua, situated at no great distance from the coast, possessing a warm climate, and a fertility of soil which can scarcely be surpassed. In it we observe every species of cultivation which distinguishes the tropics; and upon the lesser hills which rise from the valley, we observe with astonishment crops which remind us of European farming: corn grows admirably upon the heights which surround Vittoria. The valley is bounded on the north by the high land which forms the coast, on the south by the chain of mountains which separate it from Llanos, and both on the east and west a range of hills completely encloses it. By this very singular conformation, the rivers which take their rise within its enclosure have no issue, either towards the ocean or otherwise. Their waters, therefore, accumulate in the lowest part of the valley, and form by their union the beautiful lake of Tacarigua, otherwise denominated Valencia. This sheet of water, according to the testimony of M. de Humboldt, exceeds in dimensions that of Neufchatel in Switzerland; it is elevated more than 1300 feet above the level of the sea; its length is about thirty miles; and its greatest breadth does not exceed seven or eight. At the time when M. de Humboldt visited this valley, the inhabitants had long remarked the gradual drying up of the waters of the lake, which had excited their attention for thirty years. But, in truth, it is only necessary to compare the descriptions supplied by the older historians with its present state, to recognise, after allowing the largest deductions for exaggerations, that the waters have very considerably fallen. The facts themselves speak most distinctly.

Oviedo,* who, towards the end of the fifteenth century, so often traversed the valley of Aragua, positively affirms that New Valencia was founded in 1555, at the distance of half a league from the lake of Tacarigua; and M. de Humboldt found, in the year 1800, that the town was more than three miles (2700 toises) distant from its banks. The aspect of the district exhibits additional evidence of a great change. The rising grounds, which are somewhat elevated above the plain, maintain to the present day the name of islands, which, at a former period, was most accurately assigned to them, seeing they were surrounded with water. The space which has been exposed by the retreat of the waters has been transformed into most fertile fields for the cultivation of cotton, sugar-cane, and the banana tree. Those buildings which were reared in the immediate vicinity of the water are seen to be more and more forsaken by it. New islands made their appearance in the year 1796. An important military post in the shape of a fortress, which was built in 1740 in the island of *Cabrera*, is now situated on a peninsula. Lastly, in two islands of granite, those, namely, of *Cura* and of Cabo-Blanco, M. de Humboldt discovered, among the bramble-bushes, several yards above the level of the water, deposits of fine sand, containing many *helicites*. Facts which are so speaking as these, and withal so well ascertained, could scarcely fail of exciting the ingenuity of the learned on the spot, in the way of supplying explanations of the remarkable change; and they all agreed thus far, that some subterranean conduit had been opened up, which allowed the waters to flow freely to the ocean. M. de Humboldt, when on the spot, paid all due regard to this supposition, and, after an accurate examination of the localities, came very decidedly to the conclusion, that the cause of the diminution of the waters of the lake of Tacarigua was nothing more than the extensive clearing away of the woods, over the whole valley, during the course of the former half century. "In laying low the trees," he observes, "which covered the tops and flanks of the mountains, mankind, in all climates, are, at one and the same time, entailing two great calamities upon succeeding generations; they are producing a scarcity both of wood and water."

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* His Historia de la Provincia de Venezuela was published in 1723.

Since the time of Oviedo, who, like all the older chronologists, is perfectly silent concerning any subsidence of the water of the lake, the cultivation of indigo, sugar, cotton, and cocoa, had been carried to a great extent. In the year 1800 the valley of Aragua maintained a population as dense as that of any of the most populous portions of France. The smiling prosperity which existed in the numerous villages which teemed with its industrious population, could not be witnessed without the greatest satisfaction. Such was the prosperous condition of this charming country when M. de Humboldt was sojourning in La Hacienda de Cura.

After a lapse of twenty-two years, it was my lot afresh to visit the valley of Aragua. I fixed my residence in the small town of Maracay. I soon found that, for many years, the inhabitants had been remarking not only that the waters of the lake had ceased to subside, but, on the other hand, they affirmed they were very decidedly rising. The lands which had been formerly occupied in the cultivation of cotton were now submerged. The islands of Las Nuevas Aparecidas, which had risen above water in the year 1796, had now become shallows, which were dangerous for navigation. The tongue of land near to Cabrera, at the northern side of the valley, was now so narrow, that the smallest rise in the lake altogether inundated it; and a steady breeze from the north-west was sufficient to submerge the road which led from Maracay to Nueva Valencia.

The fears, which for so long a time had annoyed the inhabitants on its banks, were now altogether changed in their character; and they no longer dreaded the entire disappearance of the lake. They were now anxiously considering if these successive invasions of the rising waters were about to overwhelm their properties; and those who had explained the previous diminution, by the existence of subterrannean canals, were convinced they were now choked up, and that nothing would save them but reopening these conduits afresh.

During the two-and-twenty years which had intervened, important political transactions had occurred. Venezuela now no longer belonged to Spain. The smiling valley of Aragua had been the arena of the most bloody contests, and war and death had desolated those happy scenes, and greatly reduced the population. On the first cry of independence, a number of slaves obtained their liberty by fighting under the standard of the new republic. Its wide spreading cultivation was neglected; the forest trees, so luxuriant within the tropics, had again in a great measure usurped dominion over that region, which its inhabitants, after a century of constant and painful labour, had reclaimed. During the growing prosperity of the valley of Aragua, the numerous streams which fed the lake had been arrested and employed in innumerable irrigations, and their beds were found dry for more than six months of the year. At the last epoch to which I have alluded, the streams being no longer so diverted, flowed without interruption. Thus, then, during the progress and continuance of agricultural industry in the valley of Aragua, when the process of clearing was pushed farther and farther, and when cultivation in every shape was advancing, the level of the water gradually subsided. More lately, on the contrary, during a period of misfortune, and, we would fain hope, but temporary, when the clearing was no longer continued, and the cultivated lands have fallen back into their wild state, the waters having ceased to fall, and are now very speedily assuming a decided rising movement.

I shall now direct my remarks to another quarter, without however leaving America, in which we find a climate analogous to that of Europe, and where we traverse immense districts producing the most valuable grains. I shall direct attention to the higher lands of New Grenada, and to those elevated valleys, from 6000 to 9000 feet above the level of the sea, which enjoy, throughout the year, a temperature of from 58° to 62° Fahr. Lakes are frequent among the Cordilleras; I might easily dwell upon many of these, but shall bring under review only those which have been the subject of previous observations.

The village of Ubata is placed in the vicinity of two lakes. It is an important fact that, sixty years ago, these two sheets of water formed one only. The older inhabitants have observed the waters gradually diminish, and their shores extend themselves year after year. Fields of corn, of the greatest fertility, at the present time cover districts which thirty years ago were completely covered with water. The falling of the mean level of this lake will the more readily be credited by the consideration, that an occasional fall of three or four inches lays bare a great extent of surface. If we inquire in the neighbourhood of Ubata of any of the old men, who in their younger days were devoted to the chase, or if we examine the records of any of the different parishes, no doubt will remain that numerous forests have been felled. The clearing still goes on; and it is equally certain that the retreat of the water has not ceased, though it does not advance so rapidly as it was wont to do.

The lake of Fuquena, in the same valley, and to the east of Ubata, deserves marked attention. By barometrical measurements, made with the greatest care, I have found that its waters have precisely the same elevation as those of Ubata. It is now nearly two centuries since this lake was visited by Don Lucas Fernandes de Piedrahita, Bishop of Panama, to whom we are indebted for the History of the Conquest of New Grenada. This author, whose accuracy I have frequently had occasion to admire, and more especially as it respects distances, gives the length of the lake Fuquena at ten leagues, and its breadth at three. By a very happy coincidence, Dr Roulin a few years ago had occasion to construct a plan of this same lake, and he found the dimensions to be a league and a half in length and half a league in breadth.

It may be conceived by some, that the dimensions adopted by Piedrahita are exaggerated. But this is not my opinion; and, supporting myself on the one side by my barometrical observations, and on the other by the silence which all the ancient historians have maintained respecting the lake of Ubata, a silence which is the more remarkable, since they have described far less considerable bodies of water, I am inclined to believe that, at the time that the Bishop of Panama visited this country, there existed only a single lake, which extended without interruption from Ubata to Fuquena. In this view the calculation of Piedrahita is in no degree exaggerated. At any rate, the fact of the retreat of the waters is much more important than the estimate of the extent of surface which is left bare, a fact which is not questioned by any one. All the inhabitants of Fuquena know well that the village was built quite close to the lake, and now it is about three miles distant from it. In former times, it was an easy matter to procure timber for building in the environs of Fuquena. The mountains, which rise on all sides of the valley, used to be quite covered to a certain height with the trees peculiar to these elevated regions. There was the Cordillera oak (encinos) in abundance; and also a great many laurels (myrica), from which great quantities of wax were procured. Now the mountains are nearly entirely bare; which great change is chiefly owing to the working the salt-springs of Taosa and Enemocon. To all these authentic facts, whose number might be increased, it may be replied, that the disappearance of the water, however incontestible, might possibly have occurred had there been no clearing of the ground; and it might be contended, that their failure is owing to a wholly different, though it may be an unknown cause; and so must be ranked amongst the many phenomena whose existence is ascertained, though any thing like satisfactory explication is beyond our power.

It is true I cannot adduce here, as I did in the former case of Lake Valencia, a returning increase of the water on the suspension of the cultivation, and the renewed appearance of the woods. I may, however, procure some support for the opinion I am propounding, from the extreme slowness of the present desiccation of the valley of Fuquena, since there have been no more forests to cut down. The cultivators of the soil perceiving that there is no longer the same retiring of the waters as formerly, and a corresponding appearance of land, have been thinking of some more direct method than the clearing by which they might attain the same end. It was with this object that some speculative individuals thought of a plan by which they might drain off the whole water, by cutting a deep water-course. But, instead of dwelling on such speculative points as these, I shall here adduce a direct proof; and I believe it may be found in continued attention to the same class of phenomena we have been dwelling on. I proceed, therefore, to demonstrate, that those lakes which are so circumstanced that no clearing has ever taken place in their environs, are not subjected to any alteration of their level.

I begin with the lake Tota, because it is not far distant from Fuquena; also because these two are in very similar circumstances in a geological point of view; and finally, because it is the most curious lake that is to be met with throughout the whole of New Grenada.

The lake Tota is situated at a great height in the Cordillera of Sogamoso. Its elevation is above 12,000 feet. At this height vegetation almost entirely disappears. Here and there we perceive, scattered over the sandstone rocks, some of the plants which characterize the region of the Paramos,* viz. Saxifrages and Freglejonas, covered with a thick down, and gramineæ not unlike dry straw, which have led to the name of *Pajonales* being applied to the open savannahs. The lake is nearly circular, and Piedrahita, who visited it in the year 1652, assigns two leagues as its diameter. Its waters, when they are roused by the wind, form waves which make its navigation dangerous. According to a tradition which is much older than the discovery of America, a marine monster maintained its residence in this lake, and it had the credit of agitating the waters, and making them overflow the road which skirts its margin.

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* The regions termed by the Spanish inhabitants *Paramos* extend from a height of 1500 to 1800 toises; and are wild desert districts, exposed to great vicissitudes of weather.—Edit.

Some individuals of the highest credit have assured me, that they themselves have witnessed on the surface of the lake, not the real monster the Indians describe, but a mass of water suddenly elevate itself, and communicate as it fell such an agitation to the liquid mass, that the waves quite inundated the road which travellers are in the habit of frequenting In this account will be readily recognised a phenomenon analogous to the famous *seiches* of the Lake of Geneva. The Indians pretend that they can predict, from the appearance of the atmosphere, this agitation of the water, or, as they express it, when it is going to be angry, and hold it is then prudent to delay their journey. In the year 1652 the road skirted, as it still does, the margin of the lake, and the *seiches*, which occurred then as frequently as they do now, often made the journey sufficiently dangerous, as it is confined between the lake and a wall of elevated rocks. The waters lave the said rocks, and their level has undergone no more change than the sterile and desert country which surrounds them.

It may here, perhaps, be objected, that I ought not to have introduced, as an element in this discussion, the description of a lake which is situated on the extreme limit of vegetable existence. In the apprehension, then, that the instance I have selected, inasmuch as I regard it a striking one, may be set aside for the reason it exists in a locality composed of rocks and almost denuded of vegetation, I shall supply the description of some others, which are less elevated than is Tota, and whose waters have remained stationary for ages, although they are placed in the centre of a rich country, whilst at the same time its agricultural aspect has undergone no change. I have examined some such near the equator, in the province of Quito.

On leaving Ibarra to go to Quito, we traverse a delightful valley, in which we meet with the lake San-Pablo, to which the Indians continue its ancient name Chilcapan. I found it was elevated about 8500 feet above the level of the ocean. The temperature corresponding to this height no longer admits the cultivation of wheat or of maize, but instead, we perceive numerous fields of barley, oats, and potatoes. The lower parts of the country consist of the richest pasturage, and the hills are covered with sheep, which are reared for the sake of their wool, which supplies the extensive cloth manufactures of the district. The numerous villages which border on the lake existed even previous to the conquest; the great mass of the population is still purely Indian; they still preserve their old customs and their idioms; and, in short, matters appear in much the state they were under the empire of the Incas. The only essential difference, perhaps, which it would be possible to point out is, that the rearing of sheep has been substituted for that of the lama, although these latter animals are still by no means uncommon. On the public roads we frequently encounter droves of these lamas, under the directions of the Indians who attend them, and who by their means transport their merchandise from place to place. It is a fact admitted by every one, that the steppe of San-Pablo from time immemorial has never been wooded. Even under the Incas it was pasture-land. Folds for sheep, which were reared on the lake more than a century ago, are witnesses that the waters have in no degree receded. The route, too, which Huayna-Capac followed, when he left Quito to undertake the conquest of Otavalu, marks to the present day the limits of the water.

The Cordillera which separates the valley of San Pablo from the coasts of the Southern Ocean is covered, upon the eastern slope, with thick forests, which are almost impenetrable. I note this circumstance, because I have the strongest conviction that an extensive clearing of wood, were it to take place even on a lower level than an alpine lake, and at a considerable distance from it, would still exert an influence over the mean level of its waters.

We may here also notice, without removing from the locality we have thus introduced to notice, the singular lake of Cuicocha, which occupies a trachytic basin, in which two islands, which have been examined with much care by Colonel Hall, attest the stability and the uniformity of its level. The study, likewise, of the lake Yaguar-Cocha, or the Lake of Blood, so designated since Huayna-Capac dyed its waters with the blood of

30,000 Canara Indians whom he there slaughtered, would lead to a similar result. Neither of these lakes have any outlet. Instead, however, of dwelling upon them, I shall in preference select Chilcapan lake, and especially because it has a natural issue towards the north, whence rises the river Blanco. I wish to shew by this selection that, as I remarked at the commencement of this discussion, those observations which are made upon bodies of water with such outlets are not to be neglected. The effect we might expect to be produced by a stream issuing from a lake is, that the stream would deepen its furrow, and consequently lower the waters. I have, however, observed that, in spite of this circumstance, the level of Lake Chilcapan has not been sensibly lowered. In attentively examining the trachytic rock in the spot from which the river Blanco takes its rise, I have not been able to recognise any indication of the water producing an eroding effect. It is also true that, in the numerous cascades which I have likewise examined, I have found that a mass of water, by being precipitated rapidly, wears away the hardest rocks. But I repeat, that I have not been able to observe that the wearing effect of the water was at all appreciable when it flowed steadily over a rock, and when the water current did not draw along with it, as is very generally the case in mountain streams, pebbles, whose continual friction can scarcely fail to wear the surface of the rock over which they glide.

I shall conclude what I have to observe concerning the lakes of South America by a few remarks upon the one called Quilatoa, because it has been accurately examined at two epochs, which are sufficiently distant from each other, the one being the year 1740, and the other 1831.

In travelling to Latacunga, a town situated at no great distance from Cotopaxi, a great deal is often heard of the wonders of the Lake of Quilatoa. From time to time this lake throws out flames, which envelope the shrubs on its edge; and it likewise produces frequent detonations, which are heard at a great distance. These statements were more than sufficient to induce M. de la Condamine, who was at Latacunga in September 1738, to undertake an excursion to it. He found it was almost circular, with a diameter of about 400 yards.

The water was about 120 feet below the level of its abrupt margin.

It happened that I, too, found myself in the neighbourhood of this same lake in the month of November 1831. It cannot be compared to any thing so accurately as to a crater, the bottom of which is filled with water. I found that it was elevated 11,800 feet above the level of the sea, and hence was in the cold region. It is surrounded with immense pasture-grounds, and 1500 feet below it there are the sheep-folds of Piliputzin. To the east, the Cordillera which descends towards the coast is covered with forests which are almost unknown. The information which the shepherds who live in its vicinity gave us had little in it of the marvellous so often associated with it. They had never witnessed any flames issue from its waters; nor had they ever heard any detonations. The result of my excursion to this lake was the observation, that all things, so far as level was concerned, were in the state they had been at the epoch of M. de la Condamine's visit.

The study of the lakes which are so numerous in Asia, will probably lead to a result, in every respect conformable to that which has been deduced from the observation made in South America, viz. that the streams which water a country diminish in proportion as the clearing of it advances, and its cultivation extends. The recent labours of M. de Humboldt, who has supplied so much valuable information on this portion of the globe, seem to leave little doubt on this point. After having shewn that the system of the Altai range extends by a succession of hills, into the Steppe of Kirghiz, and that, consequently, the Oural chain is not connected with that of the Atlai, as has been generally supposed; this celebrated geographer demonstrates, that precisely at the place where we have been in the habit of placing the Alghinic mountains, a remarkable region of lakes commences which are continued into the plains which are traversed by the rivers Ichim, Omsk, and Ob. (See his Fragmens Asiatiques, t. i.) It might not be too much to say that these numerous lakes are the residue of the evaporation of a vast mass of water, which, in ancient times, covered the whole country, and which has been broken up into so many separate lakes, by the configuration of the surface. In crossing the steppe of Baraba, that he might reach Barnaoul from Tobolsk, M. de Humboldt ascertained that the process of desiccation was every where greatly augmented by the effects of cultivation.

Europe also possesses its lakes, and these we have still to examine in relation to the subject before us. My own progress through Switzerland was much too rapid to allow me sufficiently to attend to the light these waters throw on this interesting point. My regret, however, is the less severe, as fortunately a most illustrious observer has left some valuable documents which supply new proofs of the influence of cultivation upon the diminution of the quantity of water in the district.

Saussure, in his first researches concerning the temperature of the Swiss lakes, examined those which are placed at the foot of the lowest line of the Jura range. The lake of Neufchatel is eight leagues long, whilst its greatest breadth does not exceed two leagues. In visiting this lake, Saussure was impressed with the conviction that its limits at an early period must have been much more extensive; for, says he, the great level meadows, and the swamps which terminate at the south-west extremity, have undoubtedly been covered with its waters. The lake of Bienne is three leauges long and one broad. It is separated from that of Neufchatel by a succession of plains which, to all appearance, were formerly under water. The lake Morat is also separated from Neufchatel by a level morass, which no one doubts was formerly submerged. Formerly, then, says Saussure, the three great lakes of Neufchatel, Bienne, and Morat, were united in one great basin. In Switzerland, then, as in America and Asia, the ancient lakes, which we may distinguish as the primitive ones,—those which occupied the lower portions of the valleys, when the country was wild and uncultivated, have subsequently been separated into a certain number of independent ones, by the drying processes to which it has been subjected.

I shall terminate my task by availing myself, in this discussion, of the observations of Saussure upon the lake of Geneva. This interesting object was, as it were, the spot from whence the celebrated philosopher commenced his immense labours. No one ever studied it more deeply than he did.

Saussure admits that, at an epoch much anterior to the times of history, the mountains which surround the lake were buried under water. Some vast catastrophe occasioned a great disruption, and speedily the current of waters occupied no greater space than the bottom of the valley: in short the Lake of Geneva was then formed. But turning from this to the monuments which have been constructed by man, it is impossible to doubt that, during the course of twelve or thirteen centuries, the waters of the Lake of Geneva have considerably retired. This is evident from the flat shores it has left near its margin; and even in the town itself, the *Quartier de Rive*, and the low streets, have been built upon such sites. This lowering of the surface, continues Saussure, is not the result only of the wearing down of the channel whence its waters issue, it has likewise been produced by a diminution of the quantity of the waters which flow into it. The general result which may be safely drawn from the observations of Saussure is, that during the period of twelve or thirteen centuries, the running streams have gradually diminished throughout the districts in the neighbourhood of the Lake of Geneva. And there is no one, I believe, will dispute that, during that long period, there has been vast clearing away of wood, and a rapidly increasing advance in the cultivation of this beautiful country. Upon the whole, by the examination of the levels of lakes, we have arrived at this conclusion, that in those countries which have been extensively cleared, it appears very probable that there has been a diminution of the running streams which flow through the district; whilst, on the other hand, where no great change has been effected in this way, the streams have been subjected to no variation.

Great forests, therefore, in the point of view we are now regarding them, appear to have the effect, first, of preserving the volume of water destined for the use of machines and of canals, and then to be an obstacle to the rain water collecting and running off with too much rapidity; being at the same time an obstacle to evaporation. That a surface covered with trees will not be so favourable to evaporation as a well wooded one, is what every one will admit, without discussion. But that the difference of these two conditions may be adequately appreciated, it is necessary that the traveller should successively pass through a country which has been cleared, and one which has not, some time after the rainy season is over. It will then be seen that the portion of his journey in the forest is still covered with mud, while those in the open country are completely dried. It is especially in South America that the obstacles to evaporation, in a region shaded with thick forests, are conspicuous. In these situations the humidity is perpetual, even long after the rainy season is passed. The paths which are formed through them are, during the whole year, nothing better than sloughs; and the only method of drying these forest roads, is to make them as broad as 200 or 300 feet, which, in fact, is a mode of clearing them.

When it is once admitted that the running streams are diminished, as the result of clearing, it may then be important to examine whether this diminution arises from the quantity of rain being lessened, or from the greater evaporation, or finally if it be owing to irrigation.

I have already admitted, at the commencement of this paper, that it was nearly impossible to assign any exact proportions to these different co-operating causes. I shall, nevertheless, in conclusion, endeavour to appreciate their respective influence in a general way. And the discussion will subserve one important object, if I prove that there may be a diminution of the running streams from the clearing alone, without the simultaneous action of the other causes.

First, With regard to irrigation we may remark, that it is necessary to distinguish between the case where extensive cultivation takes the place of a forest, and that in which a sterile district which was never wooded becomes cultivated under the efforts of human industry. In the former case it is probable that the irrigation will contribute little or nothing in effecting any alteration in the mass of running water; for it must be generally admitted that the quantity of water consumed by the vegetation of any given surface of forest, must equal, if it does not exceed, that which would be absorbed by an equal surface devoted to culture after it has been cleared. From this it follows, that the influence exerted by this cultivated district corresponds to the condition of lands which have been cleared, acting solely by favouring far the evaporation of the rain-water. In the latter case again, that is to say, where a great extent of uncultivated land shall have been reclaimed, there will be an evident consumption of water by the vegetation which has been there promoted; and the introduction of agricultural industry will, under these circumstances, tend to diminish the water-courses which traverse the country. It is very probable that we are to attribute to this circumstance the gradual drying up of the lakes which to a certain extent gauge the running streams of the north of Asia. It is almost useless to add, that, under circumstances of this nature, the effect produced by the simple evaporation of the rain-water is not augmented; on the contrary, it ought to be rather less, for on a soil covered with plants water will not so readily evaporate as on one destitute of vegetation.

Again, in the considerations I have supplied concerning the lakes of Venezuela, of New Grenada, and of Switzerland, the disappearance of a part of the flowing streams, which are tributary to these lakes, might be attributed simply to a more limited fall of rain; whilst, on the other hand, with quite as much reason, it might be maintained it was the consequence solely of the more rapid evaporation of the rain-water. Beyond doubt there are circumstances under the influence of which the diminution of the streams is the result simply of a more active evaporation. I meant to have produced a good number of examples bearing on this point. But in a discussion of this sort it is not so much numerous, as well authenticated facts, that it is important to supply. Influenced by this consideration, I shall limit myself to the production of two facts; the one derived from M. Desbassyns of Richemond, who observed it in the isle of Ascension; and the other drawn from my own notes, being one of the observations I made during a sojourn of many years at the mines of Marmato.

In the island of Ascension a beautiful spring has been noticed situated at the foot of a mountain which was originally wooded; by degrees the spring was less copious,—at length failed; during this process the forests were cut down, and the mountain was cleared. The disappearance of the spring was attributed to the clearing. The mountain was again planted, and after a few years the spring reappeared, became gradually more productive, and finally was as copious as ever.

The metalliferous mountain of Marmato is situated in the province of Popayan in the midst of immense forests. The stream of water upon which the stampers are placed is formed by the union of many small brooks which take their rise on the plateau of San Jorge. The whole environs of the establishment are thickly studded with wood. In the year 1826, when I for the first time visited these mines, Marmato consisted of some miserable huts possessed by a few negro slaves. In 1830, the epoch at which I quitted this locality, Marmato exhibited the most exhilarating appearance. There were now seen great work-shops, a foundry for gold, and powerful machines for the division and amalgamation of this precious metal. There was now a free population of nearly 3000 inhabitants settled on the mountain side. All this implies that the wood had been extensively cut down for the manufacture of the machines, the construction of the buildings, and the preparing of charcoal. That it might be the more easily carried, all this was done upon the plateau of San Jorge itself. The clearing had been going on for scarcely two years, when it was noticed that the quantity of water which was required for the machines had conspicuously diminished. The volume of water is in fact measured by the work which the machines perform; and trials by gaging at different times have likewise proved the diminution of the water. But this is at Marmato an all-important subject, for a diminution of the fluid moving power is always followed by a diminution in the production of gold.

In these two cases, of Marmato and Ascension, it is not at all probable that an extent of clearing so local and limited could have such an effect upon the meteorological condition of the atmosphere, as in any degree to vary the annual amount of the rain which falls throughout the country. But the question need not be left in this uncertainty. At Marmato, as soon as the diminution of the supply of water was ascertained, a rain-gauge was established; and it was found, by the observation of the second year, that a greater quantity of rain had fallen than during the first, although the clearing had been continued, and there was no appreciable increase of the quantity of water at the wheels. Two years of hydrometrical observations are sufficient even in the tropics to exhibit the variation in the annual quantity of rain; and the observations at Marmato establish that the mass of running water has diminished at the very time that the quantity of rain had increased.

It is then probable that local clearings of no great extent may diminish the copiousness of springs and rivers, and even cause them to disappear, and under circumstances where these effects can in no degree be attributed to a diminution of the fall of rain.

Finally, We have still to examine if the extensive clearing of forests, extending over considerable districts, has any effect in making the rain less copious? In reply, we remark, that it is only hydrometrical observations that can lead to the solution of this question. And unfortunately the observations of this sort, which might be at our command, do not reach back far enough; and, so far as Europe is concerned, they were not commenced till the whole clearing process was well nigh over. The United States of America, however, where the forests are disappearing with astonishing rapidity, may perhaps supply the required data at no distant period.

In studying the phenomena of rain under the tropics, I have at length formed, in connection with this question of clearing, a very decided opinion, which I have freely communicated to many. I regard it certain, then, that a very extensive clearing diminishes the annual quantity of rain which falls upon a country.

It has long ago been remarked, that, in equinoctial regions, the epoch of the rainy season returns every year with astonishing regularity. This is most true; whilst at the same time this meteorological fact ought not to be announced in terms too general. There is the greatest possible regularity in the alternation of wet and dry seasons, in those countries whose territory is very much varied. Thus, a country which at once exhibits forests and rivers, mountains and great plains, lakes and extensive table lands, will at the same time exhibit periodic or changing seasons with a regularity which is quite remarkable. This, however, is no longer true if the territory be more uniform, and if it become in any way peculiar. The epoch of the return of the rainy season will be much less regular if the face of the country be exposed and arid; also if cultivation to a great extent has partially taken the place of forests; and finally, if the rivers are less common and the lakes more scarce. The rains will then be much less abundant, and, in such countries, droughts of long duration will from time to time be experienced. If, on the contrary, thick forests nearly entirely cover the whole territory, if the rivers are numerous, the cultivation be but limited, then the irregularity of the seasons will still manifest itself, but with quite a different character. Rain will then predominate, and in some years it will become, so to speak, continual.

The continent of America presents to us, in immense extent, two regions which are placed under the same conditions as to temperature, and in which we successively meet those circumstances which are the most favourable to the formation of rain, and those which are of directly opposite character. In leaving Panama, and travelling towards the south, we pass the Bay of Cupica, the provinces of San Buena Vantura, of Choco, and of Esmeraldas. In this country, covered with thick forests, and furrowed by a multitude of rivers, the rains are almost unceasing. In the interior of Choco no day passes without rain. On the other side of Tumbez, towards Payta, an entirely different set of objects present themselves. The forests disappear, the soil is sandy, and of vegetation there is scarcely a vestige. Here rain, so to speak, is unknown. When I was at Payta, according to the testimony of the inhabitants, it had not rained for seventeen years. This want of rain is common in all the countries which border on the desert of Sechura, and extends as far as Lima. In these countries rain is as seldom seen as are trees.

Thus, in Choco, whose soil is covered with forests, it rains continually; on the coast of Peru, the soil of which is sandy, devoid of trees, and destitute of verdure, it never rains; and this, as I have already said, under a climate precisely the same as to temperature, whose exposure and distance from the mountains is very nearly the same. Piura is not at a greater distance from the Andes of Assuay, than are the humid plains of Choco from the western Cordillera.

The facts which I have detailed in this memoir seem to establish, 1st, that the extensive clearing of a country diminishes the quantity of running water which flows over its surface; 2dly, that it is impossible for us to determine, at present, whether this diminution is owing to a smaller annual quantity of rain, or to an increased evaporation of the surface-water, or to those two causes combined; 3dly, that the quantity of running water does not appear to have varied in countries which have not been subjected to any changes arising from the progress of cultivation; 4thly, that, independent of the preservation of surface water, forests husband and regulate their flow; 5thly, that cultivation, when established in an arid country, which is not covered with forests, dissipates a portion of its running streams; 6thly, that, in clearings which are purely local, springs may disappear, without there being any ground to conclude that the annual quantity of rain has diminished; and 7thly, that, drawing our conclusions from the meteorological facts collected in equinoxial regions, we may presume that the extensive clearing of a country diminishes the annual quantity of rain which falls upon it.

On the Tree which produces the Gamboge of Commerce. By ROBERT WIGHT, M. D. *With Remarks, by* Dr GRAHAM.

I AM induced to request a place in the Madras Journal, for the insertion of a few remarks on the tree which produces the Gamboge of commerce, in consequence of the following observations on it by Dr Graham, Professor of Botany in Edinburgh, communicated by him in a letter dated 12th March 1836: "In consequence of having received specimens from Mrs Walker of the tree which in Ceylon yields gamboge, I have been attending to the subject lately, and on Monday last, read some observations to the Royal Society of Edinburgh about it. I have been obliged to dissent wholly from Arnott and you, that it is the *Xanthochymus ovalifolius*, and Arnott now agrees with me so far, but he has fallen into at least as great a blunder. It is undoubtedly, as I think, the *Garcinia* (Mangostana, Gært.) *Morella* of Desrousseaux and Gærtner. Arnott now thinks it *Garcinia Zeylanica*, which it cannot be, if Roxburgh describes this with any degree of truth. In fact the *Garcinia Morella*, which I have said it is, is no *Garcinia*. Murray says the tree is the *Stalagmitis cambogioides*, but his description will not apply to my plant, from which I have a great quantity of excellent gamboge. I have sent a specimen to Mr Don, t request that he will compare it with the specimens in the Banksian Herbarium, from which Murray's description was taken. If the same, the generic name *Stalagmitis* may yet be retained, and the description only altered. If not the same, it must form the type of a new genus, to which I find *Garcinia elliptica* of Wallich also belongs. It is especially characterized by the stamens, of which I send you a figure."

The point on which Dr Graham finds it necessary wholly to dissent from us, is thus briefly stated at page 102 of the Prodromus: "There can now be little doubt of this (*Xanthochymus ovalifolius*) being the only plant in Ceylon that yields gamboge fit for the arts, and that, consequently, the specific name of *Gambogia gutta*, Linn., ought to have been applied to this species, and not to *Garcinia cambogia*."

The evidence contained in Dr Graham's letter seems so completely to invalidate the correctness of our statement, that it might appear useless to attempt any refutation; yet I am not satisfied that he is either wholly right, or that we are wholly wrong. I do not think him right in considering the tree of which he has got specimens, as the only one that produces gamboge fit to be used in the arts, nor do I think it is the one which produces the true Ceylon gamboge. I do not think so, because it has been long and well-known that there are two sorts in use,—one from the eastward, Siam, Cambogia, China,—and the other from Ceylon; the latter considered inferior to the former. The gamboge from the tree in question, specimens of which I have seen, is apparently of the best quality, and much superior to the common Ceylon gamboge, having a fine, rather light colour, and glassy fracture. The true Ceylon gamboge is darker coloured, and mixed with dark brown spots. The Ceylon tree which produces the fine gamboge is rare, as Colonel Walker informs me he has only met with it in one place, and that an old garden near a former Dutch settlement, not far from Negombo. It cannot surely be supposed, that a tree so exceedingly rare as this is represented, can be the one which affords all the gamboge produced in that island; still less so, when it is borne in mind, that that obtained from it differs in quality from that usually produced there, and known in commerce under the name of "Ceylon Gamboge." From these facts, I think, we are entitled to conclude, that Dr Graham has drawn a wide inference from insufficient data, or, in other words, has attempted to form a general rule from a solitary example. I do not, however, wish it to be supposed that I insist on our statement being held strictly correct; because a degree of uncertainty attaches to the tree or trees from which this substance is procured, that all the efforts of botanists for the last century have been unable altogether to remove. All that I have attempted, or indeed wished to prove is, first, that the facts adduced by Dr Graham are not sufficient to invalidate our position, that the *Xanthochymus ovalifolius* is the only *indigenous* plant in Ceylon that produces gamboge fit to be used in the arts; though I fear, from further inquiries, that we were premature in hazarding so strong a statement; and, secondly, that the tree, from which Dr Graham's specimens were procured, is of exotic origin. I shall now attempt to account for the appearance in the island of that tree which is neither a *Garcinia* nor *Xanthochymus*.

About the beginning of the seventeenth century, the Dutch first imported gamboge into Europe from China, and, not long after, they expelled the Portuguese from Ceylon, and formed settlements of their own there, which they retained until near the end of the eighteenth century. Is it at all unreasonable to suppose, that, in the course of that long period, they should endeavour to procure for their own territories a lucrative article of commerce, in place of having to purchase from others all of the finer sorts required for their European trade? If not, we may readily suppose they imported the plants above referred to, and which have remained unnoticed by the English, until Colonel Walker accidentally discovered them about two years ago, in just such a situation as one might expect to find introduced trees, namely, in a garden close by a Dutch settlement. A most interesting discovery it is, since it seems to prove that they are of exotic origin; that the soil and climate are suitable for its growth and propagation, and leaves room to infer that it might be introduced with success on the west coast, at least, of India, the climate of which corresponds in many respects with that of the south-west coast of Ceylon; and, lastly, because it in part, at least, sets this long-agitated question at rest, by making us acquainted with the probable source of the best gamboge used in the arts.

Botanically considered, this plant presents some points of considerable interest, which may be the means of directing more of the attention of botanists to the peculiarities of the order to which it belongs than it has hitherto received.

Dr Graham shews that his plant is not a *Xanthochymus* neither is it a *Garcinia*, and, unless there is an error in the description, that it cannot be a *Stalagmitis*, but that it forms a new genus, essentially characterized by its stamens, the filaments of which are united into a single square column, and the anthers one-celled, opening at the apex by a calyptra or lid, in place of two-celled, bursting longitudinally, as in all the other genera of *Guttiferae*,—characters amply sufficient to separate it from every other genus of the order.

To the conviction expressed that this new genus is undoubtedly Gærtner's *Mangostana Morella*, I can offer no objection, as I am altogether unacquainted with that plant, except through the figure, and because Dr Graham has not stated the evidence on which he grounds this conclusion; but if it should prove correct, I must acknowledge it goes far to establish the fact of its being a native of Ceylon, and, consequently, that the juice of it as well as of other trees, may be drawn for gamboge, as that of *Garcinia pictoria*, Roxb.; another member of this new genus, is in Malabar.

Here the question must for the present rest, as it can only be finally decided by reference to authentic specimens of the plant described by the older botanists (who usually paid much attention to useful plants) as the "arbor Indica gummi guttam fundens," and which has now been bandied about from species to species, till it seems to have multiplied itself into about half-a-dozen different trees; but I trust that Ceylon botanists will now be induced to take up the subject in earnest, and ascertain, by actual inspection, and the preservation of specimens, the tree or trees—for there may be several—from which its gamboge is derived, and further to determine whether the trees which have given rise to this fresh agitation of the question are of indigenous or exotic origin.—*Madras Journal*, No. 13, p. 300.

Remarks on the preceding Paper by Dr GRAHAM.

The botany of Southern India is in excellent keeping. The zeal of Colonel and Mrs Walker is unwearied, and their exertions are bringing to our knowledge a host of new and interesting plants. The enthusiasm of my excellent friend Dr Wight is not second to theirs, and, fortunately for the science, in pursuit of which he makes extraordinary efforts, the field of his operations is much more extended. Every thing which he writes will and ought to be received by botanists with deference; and therefore I am anxious to correct one or two misconceptions into which, from the position in which he is, he has very naturally fallen. His knowledge of my opinions was, at the time he wrote these observations, derived solely from a letter which I had written to him, before I was myself acquainted with all the arguments upon which my opinion might be supported. If he had read the more detailed account which I was, subsequently, able to publish in the Companion to the Botanical Magazine, vol. ii. p. 193, I feel certain that his observations would not have appeared in their present shape.

The points stated to be at issue between my friend and me, are these:—1st, He does not think me right in considering the tree of which Mrs Walker sent me specimens and drawings, as the only one that produces gamboge fit to be used in the arts. 2d, He does not think Mrs Walker's plant the one which produces the true Ceylon gamboge. 3d, He thinks the facts adduced by me are not sufficient to invalidate his and Dr Arnott's position, that the *Xanthochymus ovalifolius* is the only *indigenous* plant in Ceylon that produces gamboge fit to be used in the arts. 4th, That the tree from which my specimens were procured, is in Ceylon of exotic origin. Now, my friend will find, that, even in my letter to him, correctly, I am sure, quoted by him, I never said that my plant was the *only one that produces gamboge fit to be used in the arts;* and in my subsequent paper in the Companion to the Botanical Magazine, I expressly guard against that supposition, thinking it very probable that there may be others. I only say that this plant does yield gamboge fit to be used in the arts, and that I had received perfectly good gamboge taken from it. We do differ on the second point, and my opinion rests on the authority of Mrs Walker;—the authority of the mutilated trees which she saw;—the authority of the Ceylon Doctor which she quotes, as shewn in my extracts from her letters, published in the Companion to the Botanical Magazine;—the authority of the gamboge which she sent to me from these trees, which proved to be excellent gamboge, chemically, medicinally, and as a pigment. With regard to the third point, there is a mistake, as Dr Wight will immediately see upon reflection. I did not attempt to invalidate his and Dr Arnott's position that the *Xanthochymus ovalifolius* is the only *indigenous* plant in Ceylon that produces gamboge fit to be used in the arts, because they took no such position; but in the quotation which Dr Wight himself makes from their excellent Flora, they give it as their opinion that it is the *only plant* in Ceylon that does so; and this opinion I shewed to be an error, by proving that another plant does. I can go farther now, however, than I could do when I wrote Dr Wight, and say, that *Xanthochymus ovalifolius* yields a juice which is not, chemically, medicinally, nor as a pigment, good gamboge. As to the 4th point on which Dr Wight thinks we differ, namely, as to whether my plant be a native of Ceylon, he will find that I never have asserted that it is, because in this country I can be no judge, and I have no direct information upon the subject. Though, therefore, I have no interest in this question in support of any thing I have written, I may add, that Dr Wight thinks that if my plant shall prove to be the *Mangostana Morella* of Gærtner, it will go far to establish the fact of its being a native of Ceylon. Now, my opinion as to the identity of these plants is founded upon the excellent figure of the fruit by Gærtner, and a careful examination of synonyms by authors who were in situations which enabled them to judge. With respect to the fact of the plant being a native of Ceylon or not, however, I long since wrote to Dr Wight, requesting he would make inquiry, and knowing his activity, and confident in his accuracy and judgment, I shall at once adopt the opinion which he shall form after inquiry. I shall be surprised if he finds it as scarce as he states it to be, in his observations in the Madras Journal; because Mrs Walker informs me, that, after knowing it in one situation, she passed through " a forest" of it in another. This expression, however, may not have been intended to be understood literally, and the two situations, for any thing I know to the contrary, may not be far distant. No man is more able to inquire into this than Dr Wight, and therefore I am confident we shall know the fact before long [*]. Borneo, Singapoor, and Rangoon, all yield gamboge, and so do probably many other places in the east, as well as Siam. Let us hope that we shall soon get information as to the plant in each which affords it. Mr Malcolmson has obligingly lately given me a specimen, unfortunately only in leaf, from Rangoon, which he found to contain a purgative, yellow gamboge-like juice in its fruit. It is certainly different from my Ceylon specimens.

When Dr Wight's observations in the Madras Journal were written, he had not heard the discovery which Mr Brown's inquiries, kindly undertaken at my request, had led to,—that the specimen from which Murray's description of *Stalagmitis gambogioides* was taken, is a compound, the flowers of *Xanthochymus* being stuck by sealing-wax upon a branch of what seems my Ceylon plant. The knowledge of this circumstance would have probably modified the observations in the latter part of Dr Wight's paper.

Account of M. Fournet's Opinions regarding the Metamorphic Changes of Rocks, and his Observations on the Systems of Elevation in the country near Arbresle (Depart. du Rhône). Contained in a Report made to the Academy of Sciences of Paris. By MM. ELIE DE BEAUMONT, BEQUEREL, and ALEX. BRONGNIART.

CONCERNING the *orography* of the district of which Arbresle forms as it were the centre, or, in other words, the *relief*, or various relations of form, elevation, and direction of the hills, valleys, water-courses, &c. M. Fournet has made remarks upon the following principal distributions ;—

1st, On the parallelism of the principal valleys of the south, the direction also, and the declivity of the lateral valleys, and those which may be regarded as the ramifications of the principal trunk; likewise on the course and declivity of those which are in an opposite direction to this principal valley; and of the main course of the water, both on the side towards the Loire, and on that towards the Saône. Concerning this latter river, whose course is from north to south, it is to be remarked, that its tributaries, the Jardine, and especially the Brevanne, enter it, flowing from the south-west to the north-east; and even from south to north. It is the same with the Gier in relation to the Rhone. Another remarkable distribution, and nearly of the same kind, is the convergence of all the valleys towards Arbresle as towards a great basin.

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[*] See additional information on this subject in Scientific Intelligence, Botany.

2d, When the structure and the inclination of the mineral strata, as well as the nature of the hills which confine and form these valleys, have been studied, we may then pursue these relations further by recognising that this main point, this kind of hollow where the town of Arbresle is situated, may be considered as a uniting point of three different systems, or of three distinct axes in the relief or conformation of the country; axes which M. Fournet has very clearly determined by the mammillary tops and the summits which characterize them, by the directions which they follow, and the localities which they traverse,—characters which are very distinct upon a map, but of which no description can convey a clear idea.

Since the time when M. Fournet gave in his work to the Academy, having assiduously prosecuted his observations on the relief of the soil, he has discovered a fourth axis of elevation, that, namely, which has uplifted the Lyonese Mont d'Or. This axis is not very apparent in the Lyonois portion, but when studied with all those helps which the theory of geological elevations supply, M. Fournet very clearly demonstrates that the Lyonois Mont d'Or ought to be referred to the elevation of the Mountains of Pilâte, the first of the Jura elevations, and differing from it only in degree.

It is upon the three first axes, indicating three systems or epochs of distinct elevation, that M. Fournet most insists, by collecting all the facts which agree in establishing the reality of this succession of geological phenomena. These proofs are drawn from the nature of the rocks, from their inclination or direction, and from the nature of the metallic veins and rocky dikes which traverse them.

The first orographic axis, which runs N.N.E., is composed of two zones of crystalline rocks, but with a stratified structure on the great scale; that is to say, of rocks whose soluble elements, in precipitating themselves into the crystalline form, were deposited tranquilly, after the manner of a sediment. The first zone of these rocks is composed of gneiss, consequently of felspar and mica; and the second also of mica, but combined with quartz and talc, the elements of mica-schist and of talc-schist which compose this zone. These rocks being stratified, their inclination and direction are appreciable; and they are, moreover, the same throughout their whole extent: their formation and their derangements likewise, by elevations, are owing to the same cause, and are principally to be attributed, according to M. Fournet, to the appearance of granite.

The second system of mountains, or the second axis of elevation, which runs nearly north-west, and so crosses the former, is principally composed of rocks which belong to the transition series. They are compact limestones, rocks of the greywacke series (*traumates*) with vegetable markings, conglomerates, &c. M. Fournet conceives that he can refer this axis or line of direction of uplifting forces to the second type of elevation established by our colleague M. Elie de Beaumont, adducing as examples the round eminences of the Vosges and the hills of Bocage. It is in the ancient depressions of this system that the coal formation presents itself, like so many islands, and here, too, it is that the metallic and metalliferous veins are found. M. Fournet attributes this axis to the elevation of quartzose porphyries.

The third axis or system of elevation which shews itself even to the north of Tarare, or the valleys of Tardive and of Azergue, runs straight from south to north: but it appears to us much less satisfactorily determined than the previous ones, both as it regards the rocks which compose it, and also respecting those which have upraised them. According to the author, it is owing to the consequences of the eruption of quartzose porphyries, but under other declinations, and has been terminated by the eruption of the trap-rock, consequently augitic or hornblende which M. Fournet, along with M. Voltz, has denominated *Minette*.

The author next inquires into the causes, or rather what are the kinds of rocks which, ejected from the bowels of the earth, have upraised, by their eruption, the stratified rocks, that principally compose the first axis, and which have broken up and overturned the mechanical rocks (*roches clastiques*) that form the chief mass of the second type. He has also recognised, in the district he describes, a great number of granitic and porphyritic masses, and of traps, which, by their massive structure on the great scale, by their penetration, sometimes in great dikes, and sometimes in simple veins across the mass of elevated chains, and even in the midst of the coal formation (as at Sainte Foy l'Argentière), indicate both the cause and the epochs of these elevations.

The rocks which the author describes as constituting the principal part of these formations of eruption or of elevation, are common granites, porphyritic granites, which have evidently preceded the quartzose porphyries, and the rock which M. Voltz has elsewhere described under the name of *Minette*,—a rock which may perhaps be referred, without any new designation, to that which one of us, the reporters, has long ago denominated *spilite*.* At the same time the minette does present, in its varieties and modifications, some peculiarities which do not belong to the rocks known under the names of *whinstone*, *blatterstein*, *variolite*, *corneenne* (aphanite), and *spilite*. These differences are owing to the presence of bronze mica, and, in some cases, to the abundance of acicular hornblende. This rock, of which M. Fournet supplies a minute account, appears to have been the last which appeared, and to have produced the elevations from north to south, or that which constitutes the third axis. Finally, in decomposing first in a spheroidal shape, and then into clay, the minette exhibits new and additional relations with the basalts and the spilites.

Metallic separations, sublimations, and productions, so designated severally according to the notions entertained of their mode of formation, also present themselves in the systems of elevation; not indeed as having contributed with the preceding rocks to produce them, for they cannot be ascribed to this cause, but, if we may so speak, as having availed themselves of the great fissures which were produced by the eruption of these rocks in the crust of the earth, and so insinuating themselves into these numerous clefts, they have there formed metalliferous veins, which assist in the most striking manner, by the difference of their nature and direction, in determining the distinctness of the several epochs, and the independence of the various elevations. For, in the first elevations, the axis of which runs from north-east to south-west, we find veins of cupreous minerals running in this direction. In the second system, which runs nearly from north-west to south-east, it is chiefly veins of quartz, of barytes, of fluor-spar, and galena, which are found parallel to this direction. It is then apparent, as stated by M. Fournet, that there are the most striking relations on the one hand between the systems of elevation, and the nature of the rocks which compose the uplifted strata, and especially the upraising rocks themselves; and on the other, between the directions of the fissures into which the several minerals, crystalline, earthy, and metallic, are as it were injected or secreted, and finally in the very nature of these minerals themselves.

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* The *spilite* of Brongniart consists of a basis of aphanite, containing imbedded portions and veins of calcareous spar, besides various other accidental minerals—in short, a variety of amygdaloid.—Edit.

Another consequence which M. Fournet deduces from his researches, and which appears to us exceedingly striking, should future observations and a more minute examination of the rocks confirm the view, is the connection he imagines exists between the silica, which is the negative element in these rocks, the different degrees of their fusibility, and the epoch of their appearance. Thus, according to him, the rocks which have first appeared in upraising and dislocating the soil, are the common granites; rocks which, being the most infusible on account of the silica, the negative element which they contain in excess, required for their melting the most direct application to the source of heat. To these rocks succeed, and in the exact order of the diminution of their silica, and the augmentation of their fusibility, the porphyritic granites or granites containing large crystals of felspar, and quartzose porphyries, then the micaceous eurites, rocks which are almost entirely composed of felspar, and finally, the *minette*, a very fusible augitic and hornblendic rock, and which might traverse all the preceding rocks without being in a solid state.

Here, then, as the result of those laborious and learned researches, we see very remarkable phenomena and consequences exhibited in the district round Arbresle. We find the epochs of elevation accurately determined; also relations established between the eras of these elevations, their direction, the nature of the upraised strata, of the rocks which uplifted them, the veins which are met with, and even respecting the chemical nature, the degree of fusibility, and the epoch of the appearance of the rocks of eruption.

These investigations form the first portion of the memoir of M. Fournet, and, according to our views, they constitute the most important part of it. But this able chemist as well as geologist, has endeavoured to push much farther the study of the effects which the upraising rocks, at the moment in a state of high temperature, produced upon the strata which they traversed; and from this examination he has drawn new conclusions, which, if they cannot be regarded as so well established as the former, are supported at least by very curious facts, and manifest the great acuteness of the observer.

M. Fournet imagines that argillaceous slate is the true and the only original sedimentary rock, and that this rock, which contains the elements of mica, and frequently plates of that mineral itself, being altered or modified in different ways, either by new combinations or by the expulsion or diminution of one of its elements, or finally by the introduction or the combination in the previous mass of certain new elements, it may be so transformed into gneiss, mica-slate, clay-slate *(phyllade)*, &c. If M. Fournet had been content with announcing these views as *possibilities* which were deduced from chemical action, we should have listened favourably to the hypotheses offered by a man so celebrated as a metallurgic chemist, and probably would have taken no notice of them in this report. Since, however, the author has endeavoured to support them by adducing facts, we must attempt to determine the value of these facts.

M. Fournet first dwells upon the varieties which this same stratified primordial rock presents in the different portions of its substance, and he inquires if it be rational to admit that the liquid compound which deposited this rock had every instant changed its composition so as to vary its deposit, and if it be not much more simple to admit that some external and variable influence has produced this change upon deposits which were originally made in a manner which was wholly homogeneous. Besides these local observations which demonstrate this influence, there is a more general one which more decidedly establishes the matter; it is, that the alteration is both greatest and most varied in proportion as the modified slate approximates to the erupted rocks.

The author enumerates four modes in which the original argillaceous slate is altered, which, affected by the influence of the burning, and especially the liquid rocks of eruption, have transformed it into products which, by their appearance and composition, are very different among themselves, and not less so from the original argillaceous slate. *Calcination* is one of these modes. Thus, in contact with that trap and hornblendic rock, which he denominates minette, M. Fournet has observed that the argillaceous slates are baked into porcelain jasper (*thermantide*). This operation, though not quite so simple as he states, many circumstances requiring notice before the result can be accurately traced, appears nevertheless to be quite true. The second mode is what M. Fournet calls *brazure*, or the method of *trituration*. This is principally seen in the transition formations. The siliceous argillaceous rocks which enter into their composition have been first broken, and, as it were, triturated by the elevation of the plutonic rocks; then these fragments, surrounded by the fusing matter, have had their angles and edges blunted by commencing fusion, and after this they have been finally soldered by this same cement. The rock which is thus produced exhibits itself under the form of a siliceous trap tuff, a kind of breccia so common in the transition formations. An example of this change may be seen in the Mouette vein near Chessy.

Other methods become more difficult of comprehension, and consequently of admission, such as those by which the argillaceous slate has been changed by fusion and subsequent crystallization into mica. In support of this proposition, M. Fournet adduces, 1st, The argillaceous slate of the mountain of Bel-Air, above Tarare, which exhibits in the vicinity of porphyritic masses many modifications of the rock in the form of bronze mica, and fine chloritic mica, containing crystals of felspar: this rock again becomes argillaceous slate when it is beyond the influence of the porphyries. 2d, A similar observation has been made by M. Mitscherlich in the Eifel. 3d, The case in which the argillaceous slate is modified into chlorite, when it has been, as it were, plunged into the liquid, or into the paste which has been crystallized into quartz; the example which he quotes of this singular change is taken from a gallery in the mine of Saint Bel. "In the fourth case (that of the bridge of Gassie on the road from Chessy to L'Etrat) the fragments of grey argillaceous slate which are found in contact with the quartz porphyries, after having undergone various alterations, have been definitely changed into beautiful dark green hornblende crystals. All the schisto-crystalline rocks of the country are either hornblendic or micaceous. The two preceding modifications give us the key to their formation by a simple softening, and, adding to this the subsequent facts regarding cementing, they easily explain to us the most part of the other changes which the argillaceous schists have subsequently undergone." These facts are so remarkable, and their connection with the conclusions the author draws are so bold, that we have thought it best expressly to quote from the memoir.

A fifth mode is that which the author denominates *change by penetration and cementing*. Thus he regards *arkose*, an aggregate rock composed of quartz and felspar, as resulting from a sandstone being penetrated by a euritic paste, that is to say, a felspar one.

The last and the greatest change which the author contends for, is that which the argillaceous schist undergoes when subjected to the influence of granites: this first and powerful rock of eruption, by producing mica with the argillaceous slate, by introducing felspar between its laminæ, transformed this slate into gneiss; and not upon a single point, or even a small space, but throughout an entire mountain. This gneiss, then, according to the apt expression of the author, is nothing more than a felspathised slate (*schiste felspathisé.*).

M. Fournet attributes to water in a state of incandescence, a very powerful dissolving and penetrating influence in effecting those modifications which are so extensive that they may almost be denominated complete transformations; and, in fact, it was quite necessary that he should admit, in a liquid so abundant and so common, a condition which was altogether extraordinary, ere it could effect transformations which, in the existing state of nature, appear to us as anomalous and astonishing as incandescent water itself.

Many of these views concerning the transformation of rocks by the agency of powerful geological operations, whether more actively, or more slow and gradually, have been proposed by different geologists, but in a manner which appears much more hypothetical, since they were not supported either upon adequate observations or upon chemical data. It may almost be asserted that the ideas concerning the transformation and passing of one rock into another, belongs to that class which suggests itself to every one; but they have rarely been based upon a critical and serious examination, and when the proof is demanded, nothing but the most vague generalities are almost invariably supplied. It is thus that both Hutton and Playfair have endeavoured to explain the formation of granite rocks by the solidification, through means of the high internal temperature of the earth, of the sediment conveyed by the course of waters to the bottom of the sea. Sir James Hall suggested that fusion under high pressure might account for the formation of certain rocks in which the gaseous elements were still retained. Dr MacCulloch, and more recently M. Virlet, have since prosecuted the same subject. The first learned geologist, ingenious observer, and able chemist, made and published in his work on the Scottish Isles, and in papers in the Transactions of the Geological Society, observations concerning the attraction, and the mechanical, and likewise the chemical transformation of rocks, which, being nearly of the same character as those contended for by M. Fournet, strongly support the results deduced by this latter gentleman, and prove, by the agreement in the facts and the explanations, that these are not quite so hypothetical as we might be led to suppose.

But the previously collected facts which led to the admission of these singular transformations of rocks, and the hypotheses they originated in their explanation, are generally much more vague than the theory which M. Fournet has deduced from his observations. The ideas of the greater number of philosophers and geologists who have preceded our author, with the exception perhaps of those of Sir J. Hall, and Dr MacCulloch, originate from very different views, and are not at all supported upon facts of equal value with those supplied by him. The conclusions at which our author arrives, and the theories which he deduces from them, have therefore appeared to us founded upon far more numerous observations, and much more special and precise than those of his predecessors. For example, we may mention, that there is in the work of which we are giving an account, a suite of the most detailed observations, made upon twenty-six kinds of rocks, which are traversed by the great draining gallery of the mine of St Bel; and these demonstrate a perseverance and a sagacity in M. Fournet which has been equalled but by few geologists.

Two classes of considerations, as we have already remarked at the beginning of this report, present themselves in the review of this very important work of M. Fournet; the former relate to the configuration of the surface, to the lines of soulevement of its elevations, to the epochs at which these elevations occurred, and to the rocks which produced them. The latter are intended to demonstrate that many of the rocks which actually appear to be so different from each other, are really derived from the same rock, and that they owe their differences to the action of the plutonic rocks. We have nothing to object to the former series. One of our number who has had an opportunity of observing some of the phenomena which the memoir so minutely describes, can vouch for the great accuracy of the descriptions; and this claims our confidence for the others. Wheresoever the alleged relations are admitted, the merit of their discovery must be wholly conceded to M. Fournet.

As to the second series of considerations, viz. that which relates to the modifications stratified rocks have undergone from the rocks of eruption, the generalization of their results may appear to us somewhat hazardous and bold; at all events, some of them; but the many facts which have been observed still remain; and the deductions though bold, are yet rational and admissible. It is for the future to determine whether the ideas of this able geologist, are in accordance with the natural processes which he admits, and which he continues to observe with the most laudable perseverance.

Remarks on the Scenery, Antiquities, Population, Agriculture, and Commerce of Central European Turkey. By Dr A. Boué. Communicated by the Author in a Letter to the Editor.

Part I.—*Scenery and Antiquities.*

European Turkey contains numerous gulfs, valleys, and mountainous districts, whose magnificent landscapes well merit the attention of travellers, although unfortunately they are but too little known. We often hear of painters wishing to have new objects for representation. Now, scarcely any part of the eastern world has as yet been explored by artists; for it is only some of the maritime parts which have been properly illustrated, whilst the interior of the country still remains almost a *terra incognita* to painters. Yet not only would the landscape painter find ample materials for his pencil, but the portrait painter would find a still richer treasure in the non-European faces, forms, dresses, and manners. He would be enabled to compose pictures, in which the finest ancient Greek faces and forms would be intermingled with the beautiful and genuine oriental ones; and he might ornament these, in a most picturesque manner, by dresses, partly Asiatic, partly resembling the old Roman and Grecian *costumes.*

Where do we find paintings giving us a clear idea of an Albanian marriage, which offers so many singular fashions? Where can we see a good representation of the *Kolo* dance, as performed by the Servians; or of those dances, executed within view of the snowy tops of the Tschardagh, by the gay Bulgarian girls, when on their way to the labours of the harvest? Where do we find a painting of a Turkish lady travelling with her armed attendants? and, Where, finally, is the picture representing a half Turkish, half Christian, travelling party; or the bivouac of such a party at an inn, or perhaps under some rock or wide-spreading tree?

For the admirers of maritime scenery, the borders of the Black Sea, and also the Archipelago, abound with views as fine as those along the coast of Liguria or Naples. I shall never forget the delight I experienced on beholding the great Gulf of Salonichi, with the hills of the Chalcidian peninsula to the east, and the snowy tabular-shaped Olympus to the west. Before me lay the large white town of Salonichi, situated in an amphitheatre, like Genoa, at the base of the last of the Chalcidian hills, and crowned by a pretty large citadel. All around were plains filled with Grecian antiquities, tumuli, and the ruins of the ancient Pella, the chief city of Philip; whilst behind me arose the first of those beautiful rocky walls which defend the entrance to the interior of Macedonia.

If we proceed a little farther into the country, at the distance of twelve leagues from Salonichi, we reach the chief of the rural beauties of Macedonia—the well-known Vodena in the valley of the Wistritza.

The luxuriant vegetation commences to the south of Ostrovo, where the road passes through a defile so well clothed with fine Mediterranean trees (green oaks, &c.), that the traveller might easily fancy himself in an English park, were it not that a small post for *gendarmes*, picturesquely situated on a projecting rock, at once puts an end to the illusion. After passing the marshy lake of Telovo, situated among the hills like a crater, we arrive at the village of Telovo, where the united waters of the lakes of Telovo and Ostrovo, are seen rushing down through travertine rocks in the midst of *Colutea*, *Cercis*, nut and fig trees.

The three cascades, of which the highest is at least forty feet, have a very striking appearance. From hence to Vodena the valley is in reality a park of Mediterranean fruit-trees, intermingled with the patches of maize. At the entrance to the town of Vodena, there are a dozen of palms (*Platanus orientalis*), under whose mighty shade there is always an agreeable coolness; but I was much annoyed on observing some Turks carelessly employed in cutting off a part of one of them. One can see, in this neighbourhood, that the ancient Greeks have not spoken in exaggerated terms of the shelter which could be afforded to a numerous assemblage of people under such high and beautiful palms, or even under a large fig tree. Under the town, which is situated on the edge of a high precipice, the waters of the Wistritza, here divided into four streams, fall into the valley in cascades, of which the highest must be seventy or eighty feet; and smaller ones also occur lower down. The view is truly magnificent, consisting of the white travertine rock surmounted by the town; the four cascades; the valley studded with fruit-gardens with all kinds of Mediterranean fruit-trees, and watered by numerous small streams; whilst, on the right and left, are finely wooded hills, rising to an elevation of from 2000 to 3000 feet. Why did the kings of Macedonia not choose such a place for their chief city, instead of placing it on the borders of a marshy plain? On the edge of the precipice at Vodena, a Grecian bishop has erected, in his garden, a *kiosk* or pavilion, from which the whole view is so well seen, that he is put to considerable trouble and expense by the frequent visits of Turkish travellers; this shews, however, that the Turks have a taste for natural beauties.

The neighbourhood of Castoria, with its circular lake, presents a different kind of scenery, which, though less lively, is still very pleasing. The town is situated on the isthmus of a barren calcareous peninsula. The stone houses, of two or three stories, arranged in the form of an amphitheatre on both sides of the isthmus, the placid waters of the lake with its small boats, the round and conical hills which encompass the lake, would form altogether a Swiss view, if the hills were covered with wood; but there, as in other parts which the Romans inhabited, the forests have been cut down, and the stranger must rest contented with the trees of smaller forms which line the borders of the lake. The town, its situation, its houses, and the paved roads leading to it, all concur in giving us the idea that we are walking on old Grecian and Roman soil. The neighbourhood of Lake Ochrida is reckoned much finer, as the scenery is wilder and better wooded.

Amongst the most magnificent scenery in Turkey is the view from the plains of Upper Albania, or the White Drina, of the rugged and snowy peaks of the Ipek chain, as also of the great

Tschardagh chain rising to a height of 9000 feet.* I cannot compare this view to any thing with which I am acquainted in Europe, but possibly something like it may exist on a smaller scale in Spain. It appears as if the chain of the Pyrenees, and a part of the alpine hills of south-east France, were placed together. The country is abundantly covered with forests and vegetation, and the green of the chestnuts and oaks, the dark green of the fir-trees, and the whiteness of the snow, give an agreeable variety to the landscape, which is, moreover, adorned by the ancient white castle of Prisrend, together with numerous villages and monasteries at the foot of the hills.

In the interior of the groups of hills, to the north of Ipek, there are numerous fine Swiss subalpine landscapes, abounding in cattle of all kinds, *chalets*, and picturesque crags, together with meadows and flowers. On the other hand, in the Despotodagh, the painter finds a great abundance of wilder scenery; such as very deep valleys and fine forests. On the north side of this chain the vegetation is similar to that of central Europe; while on the south it resembles that of the countries bordering on the Mediterranean.

Proceeding farther, we find a great variety of landscapes in the valleys of Bosnia, in the Cermitza, on the borders of the lake of Scutari, in the valleys between Nissa and Sophia, and on the borders of the Danube. In the first mentioned countries we have sometimes wild, sometimes rural scenery. To the south of Nissa the views are less extensive; they consist generally of a rich country interspersed with crags, and occasionally adorned with some old spire or monastery. On the Danube rugged walls of rock rise on each side, while the river itself sometimes assumes the appearance of a stream, sometimes that of a lake, with patches of cultivated ground, small villages, and ruins on its banks. There is a fine panoramic view of the woody principality of Servia from the Sturacz, the highest of the central hills of that country, and belonging to the Rudnik group. The traveller will also be interested in seeing Crnutscha, where Prince

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* In vol. 22, p. 52 of this Journal, the elevation was erroneously stated to be 7000 feet.

Milosch was born, and where he still has some plain country houses, as well as the small plain near it which formed the last refuge for the Servian troops, with their families, when they were beaten by the Turks.

Turkey is pretty well stored with antiquities, and localities where important events have happened. For my part, I take more pleasure in the latter, as the remains of antiquity are generally in a most defective state in that country. In these localities I still find every thing nearly in the same condition as it was when the event happened; but I am grieved and annoyed to perceive, on spots once beautifully ornamented, or crowded with inhabitants, only the debris of walls, miserable looking stones, or still more miserable-looking new buildings with their semi-barbarous inhabitants, and sometimes disfigured with the names of foolish travellers. In such cases all illusion must at once cease, for any one who has not been born a poet.

The antiquities in Turkey are Grecian, Roman, Servian, and Turkish. Among the first we may, I think, place the great *tumuli* which are found in Macedonia. They are conical hillocks of the height of 10, 20, or 30 feet, or even more. I observed two small ones between Malocelo and Pobovdol, on the road from Dubnitza to Radomir, and there are two others somewhat higher on the western side of the high road from Djumaa to Dubnitza, opposite to the beautiful village of Rilo. It appears that this village (which is situated at the entrance of one of the great valleys of the Despotodagh), or at least the cultivated valley near it, was formerly the site of a town; and coins are still occasionally found there. The same thing also sometimes occurs in the neighbourhood of Radomir, where I was shewn some Roman imperial coins. I saw a small tumulus to the W. of Langosa, and another at 1½ leagues to the N. E. of Salonichi; but the greatest number and the highest are near Pella, to the N. of the road to Salonichi; of these I counted eight. Possibly they were formerly near some high road, as there appear to be traces of pavement underneath the grass and thistles. Others must decide if these tumuli, which are not mentioned by Cousinery in his description of the neighbourhood of Salonichi, are true Grecian monuments, or what they are; but it happens, singularly enough, that there is near Altenburg (not far from Presburg in Hungary), a conical hillock of a similar form, but possibly erected for a different purpose. It would, perhaps, be worth while to open those in Turkey. In the neighbourhood of Pella, on the high road near an excellent fountain, there are what appear to be the ruins of a temple, consisting of a square room with walls of square stones, and the traces of two or three smaller rooms. It is used at present as a reservoir for the water. Several palms are close to the spot, which is like an oasis in this flat and treeless country.

At Salonichi are two well known triumphal arches built by the Romans, one for celebrating the entrance of Octavius and Antony into the town after the battle of Philippi, the other adorned with Corinthian columns, and called after Constantine. Between these two there exists an old ruinous temple, partly hidden by new buildings, which was at one time converted into the church of Peter and Paul, and afterwards into a mosque.

Probably the finest *Roman Antiquities* I saw, were near Uskup, the ancient Scopia (Justiniana Prima), consisting of the remains of an aqueduct, similar to those near Rome, with fifty-five large arches still entire. The aqueduct describes a slight curve, and the arches diminish in magnitude on both sides. They are built, like those between Mehadia and Orschova in the Bannat, half of stone, half of large bricks. Between every two arches there is always a smaller one placed a little higher, and near it there are generally the ruins of a small square tower. There is a very short Latin inscription at one end of the aqueduct. At one league's distance from Uskup there is a small hill, which, from the debris on it, appears to have been formerly the site of some town.

I observed in the vineyards on the hill above Kostendil (Justiniana secunda) the debris of walls and bricks, so that the old town was probably situated there in the time of the Romans, or at least there may have been some castle on that spot. Remains of a Roman fortification with walls (Porta Trajana) occur in the defile to the S. of Ichtiman. The position of Florina in Macedonia on the declivity of a hill, and the straight paved roads leading from it to Castoria, would indicate the possibility of their having been also Roman establishments. Sometimes there are two or even three pavements, one on the top of the other; from which it would appear that they had been repaired at different times, perhaps, even by the Turks. In Bosnia there are remains of a Roman city (Adubo) near Mostar in Herzegowina. At Vischegrad there is said to be a Roman bridge with columns.

It does not seem probable that the Romans inhabited the whole of Servia, but only some parts of it, particularly those bordering on the Danube, and the great Morava valley, through which the road led to Sophia and Romelia as at present. Every one is acquainted with the site of the bridge across the Danube constructed by Trajan near Cladova, the inscription called Trajan's Tablet opposite to Ogradina near Oroschova, and some remains of castles along the defiles of the Danube. It seems probable that the Romans used the sulphureous warm-bath at Banya, near Alexinitza in Servia, and that the Turkish building is founded on the Roman one.

Although time, and the destructive hands of the Turks and the nomades of the middle ages, have left so few remain sof Roman and Grecian buildings, there are still many ancient *Servian churches and castles.* A Gothic castle exists at Prisrend in Macedonia, once the seat of the kings of Servia. At Perlepe in Macedonia, we find, on a most singularly shaped conical granitic hill, the remains of the vast castle of Marco-Kralowitsch, one of the most renowned of the Servian heroes. The whole top of the hill, which rises about 280 or 300 feet above the plain, was surrounded by a high stone wall; and there were at least seven square towers for the further protection of the place: their remains are still visible on the summits of five hillocks, near the gate of the fortress. The ground is divided by a small ravine; on one side of which are two towers, and, on the opposite side, the others, together with the remains of seven or eight buildings, viz. the castle and a small garden, the stables, a small chapel on the edge of a precipice, a tower of refuge with a room and chimney cut out of the rock, &c. We can still see the steps cut in the stone, which were used as a staircase, and were perhaps at one time covered with wood. It is difficult to reach the highest tower, as the steps are partly worn away. The citadel had only one gate, on the side looking towards Perlepe, and the entrance to it was defended by a large square tower, within which was the only spring on the hill. From the summit of the hill there is an extensive view of the country, which is mountainous towards the north, east, and west, but flat towards the south.

I may mention, among the old castles of the middle ages in Servia, that on the hill of Avala, 2½ leagues from Belgrade, said to have belonged to a *chevalier* Portscha of Avala; and that of Stolacz, at the confluence of the Morawas, where a Servian nobleman so long withstood the attacks of the Turks, and, when finally overpowered by them, precipitated himself along with his wife into the Morawa, an heroic deed, which is still celebrated among the people. At Kusnick in the Upper Rachina valley, there are the ruins of a castle situated on a very high hill, which, from the extensive view it commands, and its vicinity to the Ibar valley, is still used as a military post in time of war. A part of the surrounding wall, with the chief entrance and five large square towers, as also some small buildings below the castle, are still visible.

The finest Servian architectural remains are the *churches* and *monasteries*. Of the former, the finest are Studenitza and Kruschevetz in Servia, and more especially, Detschiani in Upper Albania near Ipek. At Kruschevatz there was formerly the royal palace, with a chapel much ornamented in the Byzantine style. At present we can only observe some traces of a wall, and the chapel has lost part of its spires, and its roof. The Turks used it as a hay store, and made a sort of fortification on the platform of the castle. The chapel is in the form of a Grecian cross with two round towers. In the front there is one large central door, and one small lateral one. The former is surrounded with small columns, and above is a sculpture in *bas relief*, representing the baptism of our Saviour. The arched windows are also decorated with some ornaments; and the small door is ornamented with other *bas reliefs*, as also eagles with extended wings, lions, &c.

The Detschiani church belongs to a monastery situated in a secluded valley, which is separated from the Albanian plain by a forest of chestnut trees, and has, on its other side, lofty hills and forests of ten leagues in extent, which no human being inhabits. The monument built by king Stephen is in admirable preservation: he himself is buried there. The Servians have done their utmost to prevent the destruction of this relic; but the monastery seems at present to be deeply in debt, and the *Igumen* was ill, partly from grief. It is astonishing that this church should have so long resisted the destructive spirit of the Turks; especially when in the neighbourhood of their mortal enemies the people of Montenegro. It is built of a coarse white marble, and inside, it is also ornamented with red marble: it is not large, but very clean and elegant; and, being surrounded with green meadows and forests, it has a fine appearance when viewed from a distance. The form is that of a Greek cross; it is also built in the Byzantine style, with two doors, one lateral, one central; and these, as well as the projecting windows, are ornamented with sculptures or *bas reliefs*. Lions, and dragons, together with eagles with extended wings, are particularly to be remarked above the lateral door. There is only one short massive spire, and the church is less ornamented than that of Kruschevatz. The interior, like that of all other Greek churches, is divided into a portico, the church with its columns, and the altar or sanctuary. The sarcophagus containing the body of the holy Stephen is also exhibited.

Among the other churches of monasteries, I may mention the small chapel of the Vratschefnitza monastery among the hills of Rudnik in Servia,—a monument in a more simple style, but certainly as old as, if not older, than the others. The Turks have not at all destroyed it, as it is situated in a secluded and woody valley. The windows are in the form of *ogives*, with small projecting semicircular ornaments on the walls. The spire is very low, and there is a small open portico before the door. We observed similar churches, also very old, but still less ornamented, at Lisito on the Lake of Castoria, and at the monastery of the Gemini, south of Karatova in Macedonia. On a hill near Novibazar are the ruins of the old monastery of the columns of St George (Stupani Sv. Djordje), which was destroyed by the Turks.

The largest Grecian monastery in all Turkey is situated about the centre of the Despotodagh or Rilo-dagh. Formerly there were 5000 monks in these chains of hills, and this monastery still contains 150 Bulgarian monks.

On Mount Athos there are a number of monks, but the monastery is not so large. It possesses several villages, and a narrow valley twenty leagues in length. The building is situated on the upper subalpine part of a long and very narrow valley, of which it occupies the whole breadth, forming a sort of irregular square, with high pentagonal three-storied buildings on three of the sides, whilst the fourth is occupied by the garden and the bed of the torrent.

From the outside it has the appearance of a castle with thick stone walls; there are two iron doors, and at each angle of the pentagonal line a projecting tower-like building. Windows, which are of a large size, occur only in the third story, as, lower down, the chief apertures are loop-holes for musquetry. On entering the court-yard, one is quite surprised to find a fine building decorated with several fresco paintings, and provided with large wooden galleries, and with three balconies on each story, the galleries being supported by columns. The upper story contains very elegant rooms, with Turkish *divans* and carpets, and windows of glass, instead of paper, as is often the case in Turkey. There are also some allegorical paintings on the walls; in short, it is a most convenient lodging for strangers as well as for the monks. This building has been burnt down five times, and is now just rebuilt a sixth time by the Turks, with the assistance of foreign money. Besides the monastery with its hundred rooms, they have rebuilt the church in a very handsome style, viz. in the form of a cross, with three vaulted chapels, and twelve fine columns of white marble and a chloritic hornblendic rock, together with numerous gilt wooden ornaments. The Turks, with their usual carelessness, have not yet provided a fire-engine, but the monastery is divided into three partitions, by thick walls and iron gates, so that only one part of the building can be destroyed in case of fire.

Close to the church, in the centre of the court-yard, is a very old square tower, built in the time of the Servian kings, to protect the holy men against robbers. An old inscription in brick, states that it was erected under Stephen Duchan. The monks still retain some armed peasants as a protection against robbers.

The *Turkish* antiquities are few, and too often scarcely to be recognised. Near Novibazar, and at Banya near Nissa, there are Turkish baths or octagonal buildings, lighted from the top, and containing an immense basin of hot water, with two other small rooms for vapour baths. Adjoining the baths at Banya I observed an old building with a ruinous minaret; the great gate was ornamented in a singular manner on both sides, having on the one an inscription, a horse, and an ear of corn, and on the other a king and a hare. I cannot say if there are many old paved roads which owe their origin to the Turks, but most probably there are only few, although it would appear that formerly the roads were occasionally in better order, the Christians being obliged to work at them, which is not now the case. I also doubt if the stone bridges at Uskup on the Vardar, and at three leagues to the east of Kostendil on the Strymon, are Turkish: I should be inclined to think them of an older date, although they are not at all elegant, and the arches of both, especially that over the Strymon, are clumsy and sharp-edged.

Some of the old remains of fortifications are also probably Turkish: I saw some of this description to the west of Langosa. Old burying places are very common in Turkey. They are not always surrounded by walls, and are placed near the highway or on it, often very far from villages and towns. Many travellers have been inclined to regard these as the ruins of destroyed or deserted villages; but, in fact, many of them are nothing more than the burying-grounds of the nomadic tribes of Albania, who either live not very far from the burying-ground, or return to the spot, and bury their dead in the same place as their ancestors. In Servia burying-grounds are also sometimes isolated, and they do not always bury the bodies deep enough; and thus the pigs occasionally open up the graves, or at least destroy the wooded frames which are placed over them.

(To be concluded in our next Number.)

On the Cause of the Temperature of Hot and Thermal Springs; and on the bearings of this subject, as connected with the general question regarding the Internal Temperature of the Earth. By Professor GUSTAV BISCHOFF of Bonn. Communicated by the Author. (Continued from Vol. XXIII. p. 330.)

PART II.—*Are the observations hitherto made in mines sufficient to determine the law of the increase of temperature towards the centre of the Earth?*

All our former investigations have led us to conclude an increase of temperature towards the centre of the earth; no phenomena, no observations, were contradictory to such an hypothesis; indeed, without its previous admission, many were incapable of explanation. We have now to determine whether this increase of temperature follows any fixed law, and whether that law is the same departing from all points equally distant from the centre of the earth, for example, from the level of the sea. Although it would seem that this inquiry could only be made by means of experiment, by comparing observations actually made, it may yet be deemed not inexpedient, first to consider what causes may influence the internal temperature of the earth, or in general, to determine what modifications the external temperature may operate on that of the interior.

CHAP. XII.—*What influence do the meteoric waters exert on the internal temperature of the Earth?*

In Chap. VII. it has been shewn that the temperature of the meteoric waters has no great influence on that of the crust of the earth through which they flow. But small as this influence is, it will yet be found to be very various in different parts of the earth, according as the strata be more or less pervious to water, or as the waters sink to a greater or less depth. But such variations must naturally disturb any supposed fixed law in the increase of temperature towards the centre of the earth.

The uninterrupted infiltration of the meteoric waters into mines, must cause a general cooling of the surrounding rock;

for, admitting the increase of temperature towards the interior of the earth, the mean temperature of those waters must necessarily be lower than that of the mines in their lower workings, where the direct influence of the external heat can no longer be felt. But as we are not yet acquainted with any other means of ascertaining the increase of temperature towards the interior, than by observations in mines, this difficulty can never be avoided. Reich* justly remarks, that this effect of the atmospheric waters must be greater in old mines than in new ones, although in time we do not doubt it must reach its limit. To this cause, as well as to the sinking of the colder air into the mines, he is inclined to ascribe the less rapid increase of temperature observed in the *Erzgebirge*.

The observations in the *Saxon Erzgebirge* furnish many proofs of the cooling effect produced by the action of the meteoric waters on the rock in mines. Thus, Reich † found from thirty-three monthly observations, that, at a depth of 215 feet in the *Mathusalem* mine, in the *Freyberg* district, the air had the same temperature as the rock, that the seasons of the year seemed to have no effect upon it, but that after continued dry weather the temperature was somewhat elevated, and that it suffered a depression after long continued wet. This depression will of course be greater, if water brought from the surface into the mines, for putting the machinery in motion, should happen to pass near the points of observation.

In a sounding at *Rüdersdorf*, eight leagues from *Berlin*, which has been carried on to a depth of 880 feet from the surface, and 700 feet below the level of the sea, a continued increase of temperature has, indeed, been observed in the greater depths, as well as an uniformity in the temperature of the water which flows out of the sounding pipe, at the bottom of the well or shaft, which extends to the depth of eighty feet; however, from a comparison of the continued observations of more than two years, we find a considerable irregularity in the increase of temperature with the depth. The most rapid increase is observed from 200 to 225 feet deep, where, on the 4th of December 1831, the temperature varied from 56°.30 to 62°.37, giving a difference of 6.07. This circumstance can hardly be accounted for in any other manner, than by supposing colder springs to exist there, which suddenly cool the warmer waters rising from below. The mixing of these springs cannot take place immediately, as the sounding well is lined with a sheet-iron tube to the depth of 621 feet. A later observation gave this sudden increase, between 200 and 250 feet at 5.85; indeed the temperature was found to be generally lower at equal depths at the time of this experiment, than during the former. The highest temperature at the bottom of the well was 74.30.*

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* P. 133. See page 35. † P. 58.

Were a well to be sunk on *Teneriffe*, for the sake of thermometrical observation, at less than 4000 feet above the level of the sea, within which limit Von Buch found but slight variations of temperature in the springs, would not such an uniform temperature in the waters issuing forth at such unequal elevations, have some influence on the temperature of the strata situated within those limits?

CHAP. XIII.—*What influence have thermal springs on the temperature of the interior of the earth?*

In Chap. IV. it has been observed, that considerable bodies of water rising with an elevated temperature are capable of influencing the temperature of the soil. This remark holds equally good for strata situated at a greater depth. Were we obliged to confine ourselves to the consideration of those commonly called warm or hot springs, it might be objected that they are a local phenomenon, and ought, therefore, not to be taken into account. But in Chap. I. we have shewn that thermal springs are of very general occurrence; that they are not entirely wanting in any formation; and that in different places they are only more or less frequent, and more or less elevated above the mean temperature of the air, sometimes rising to the surface by natural or artificial artesian wells, and sometimes being held back by impervious strata. How various and innumerable, then, may not be the effects of thermal waters upon the interior strata of the earth?

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* Poggend. Ann. vol. xxxiii. p. 233. See also vol. xxii. p. 136, and in the "Schriften der Berliner Akademie" for 1831.

In the copper mine at *Oatfield*, at a depth of 1392 feet, and 72 feet east of the shaft, the temperature was found, in May 1822, to be 79°.92; whilst in an end, situated only 24 feet deeper, 180 feet to the west of the shaft, it was 85°.10. Here the water, which issued in considerable streams from two narrow clefts in the floor of the gallery, but a few feet distant from one another, shewed the unequal temperatures of 81°.95 and 86°.45*. In the neighbourhood of some tepid springs in the *Churprinz-Friedrich-August* adit, in the *Erzgebirge*, the temperature of the air was exactly 68°.0; and when the waters had acquired an uninterrupted afflux, they soon warmed the rock to such a degree that their temperature remained constant†. How great, then, must be the influence of considerable masses of water of an elevated temperature, such, for instance, as those of *Aix-la-Chapelle*, *Burtscheid*, *Wiesbaden*, *Carlsbad*, &c., on the rock beneath the surface.

Waters which descend into the earth through clefts operate more or less with a cooling effect, and, on the other hand, waters rising from great depths operate more or less with a warming effect, upon the rock.

Innumerable modifications may be caused in the thermometrical circumstances of the interior of the earth by various combinations of cases; we will here mention a few for the sake of example:—

1*st*, Let EE, Pl. II, Fig. 2, be the bottom of a valley, cut off from communication with the surrounding hills by the impervious strata *d*, *d*, *d*, but the soil of which admits of the infiltration of the meteoric waters until they reach those impervious strata. In this case the meteoric waters will only sink into the earth without being able to return to the surface. The soil through which these waters have passed can therefore suffer no alterations of temperature but from without, as all waters which might bring heat from below are intercepted by the impervious strata *d*, *d*, *d*.

2*d*, The contrary case will happen when the impervious strata

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* Gilbert's Annal., vol. lxxvi. p. 431.

† Reich., p. 153.

d, *d*, *d*, are wanting, and when the waters which sink through the clefts to a certain depth, *t*, (Pl. II, Fig. 3) are forced again to the surface at *h* by hydrostatic pressure.

3*d*, Let Fig 4, Pl. II, represent a mountain. The springs in the upper regions will most commonly descend through the sloping channels *ab*, *cd*, *ef*. Here, then, will be an example of case 1*st*, namely, when the interior strata can only be acted on by the external temperature. As the temperature decreases from below upwards, the strata situated nearer to the foot of the mountain will besides be cooled by the waters which descend from the higher regions. Should any part of the meteoric waters be enabled, by entering extensive fissures in the mountain, to descend far below its foot, and reappear in the plain, it would bring up heat with it to the surface. So that, according as the observations be made in the mountain or in the plain, all other circumstances being alike in both cases, the results must shew an unequal increase of temperature towards the centre of the earth.

If we consider all the cases above investigated, we see that the increase of temperature towards the centre of the earth, even if supposed in itself to follow one fixed law, must be found to be very different, according as the wells in which the observations are made happen to be sunk at the foot of a mountain, or more towards its summit, on an extensive plateau or on the slope of a hill *, whether they be encompassed on all sides by impervious strata, and whether they be immediately surrounded by high mountains or not. What innumerable combinations of these different cases may not be imagined by which the increase of temperature towards the centre of the earth might be influenced?

Chap. XIV.—*What influence has the air on observations of temperature in mines?*

That this influence must tend materially to disturb the observations is easily to be foreseen. When shafts communicate with adits, or when several shafts of unequal depth are in communication with each other, it is well known that an uninterrupted change of air takes place, the direction of which depends on the external temperature being higher or lower than that of the air in the mine. In the former case the warm air sinks down into the shaft, and the cold air escapes from the adit; in the latter the cold air enters by the adit, whilst the warm air rises out of the shaft. This change of air is not only dependent on the seasons of the year, but on the time of day, and even on the wind, which agitates the air without*.

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* On the influence which the configuration of the surface of the earth has upon the increase of temperature towards its centre, see Chap. XVII.

In the mines of *Cornwall*, a difference of 1°.125 was found at a depth of 1071 feet between the summer and winter observations†. In the auriferous mines of *Pestarena di Macugnana*, in the valley of *Anzasca*, the March and August observations shewed differences of 1°.575 in a depth of 770 feet, which did not entirely disappear until a depth of 1077 feet‡. Among the above-mentioned observations, which have been carried on since 1828 in many of the Prussian mines, holes were bored into the rock in places free from wet and draught, into which the thermometers, surrounded with a non-conductor of heat, were sunk, so that the scales projected sufficiently to enable the observer to read off the degrees without the necessity of withdrawing them. Notwithstanding these precautions, fluctuations were caused in the height of the mercury not only in the upper but also in the lower stations, by the imperfect exclusion of the air from the thermometers. From the table given in Chap. VIII. may be seen how irregular these effects shew themselves at depths of 27 to 63 feet; but even from 145 to 738 feet below the surface differences of 0.562 to 2.25 occur, the latter of which were observed in the greatest depths. The variations of the thermometers in the upper stations corresponded in some cases very regularly with those of the atmosphere, but in others they were totally different. In the latter cases the influence of the air can only have acted very indirectly. The fluctuations in the lower station seldom corresponded with those in the upper, at one time being altogether different, at another time, although corresponding pretty exactly on the whole, yet each fluctuation being greater than in the upper stations.

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* Fox communicates some interesting experiments on the effects of change of air in mines in the Transactions of the Royal Geological Society of *Cornwall.*

† Thomas Lean in the Annal. de Chim. et de Phys., vol. xiii. p. 200.

‡ Schweigger's Jahrbuch, vol. lii. p. 268, in note.

In Chap. VIII. it has been remarked, how, during the observations in the *Saxon Erzgebirge*, even thermometers, the bulbs of which were sunk 40 inches into the rock, and that at depths to which heat cannot reasonably be supposed to be communicated through the earth's crust from without, still indicated the influence of the external temperature. Indeed, at the great depth of 1014 feet, at the lowest point of observation, the effect of the air in the mine upon the thermometer, which was increased by the bulb being only situated at a distance of 30 inches from the surface of the rock,* was very great. Here the greatest difference observed during twenty-eight months amounted to 4°.77. The result of the experiments was, that out of 29 different points of observation, situated from 147 to 1014 feet below the surface, 18 were found to give evidence of the more or less considerable influence of the seasons. In one place, at the depth of 687 feet,† during three years, the highest temperature was observed in the middle of the winter, the lowest in the middle or towards the close of summer. The influence of the air could be observed with the greater certainty, in these experiments, as other thermometers, exactly corresponding with those sunk into the rock, were placed, as we have before mentioned, close by them, for observing the temperature of the air.

From all this it follows, that it is extremely difficult, or, in fact, literally speaking, impossible, perfectly to protect the bulbs of the thermometers, buried in the rock, from the influence of the seasons. This would be no inconvenience, if the effect continued, as far as the place of observation, to correspond precisely with the variations of the external temperature, provided the observations were carried on for at least a year. However, it may be supposed, with great probability, and the observations in the *Erzgebirge* seem to favour the supposition,‡ that the effect is more frequently to depress than to raise the temperature. Unfortunately the extent of this effect is impossible to be determined.

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* Reich. p. 38. † Ibid, p. 31. ‡ Ibid. p. 133.

In Chap. VI. we noticed the natural ice-grottos, and, in general, all points which possess a temperature lower than the mean temperature at the surface. We then asserted, with great plausibility, that their number might easily be increased with a little attention. Several philosophers, and, among others, Kupffer*, have wished to explain this phenomenon, by supposing that the cold air which sinks into such caverns in the winter, cannot, by reason of its greater specific gravity, easily reascend during the summer, or, at least, that it only gives way for other colder layers. Although this explanation will not suffice in all cases, yet in many it may be admissible. This granted, the observations on the increase of temperature in the earth would suffer material interruptions in places where circumstances are similar to those necessary for the existence of ice-grottos. The above-mentioned observations in the *Erzgebirge*, furnished exemplifications of this fact.

CHAP. XV.—*What influence has climate on the progression of the increase of temperature towards the centre of the Earth?*

Although circumstances of climate may cause differences of several degrees between the mean temperature of places situated in the same latitude, at the same elevation, and at no great distance apart, yet it cannot well be supposed that the influence of such variations would be felt to any great depth in the earth's crust, indeed it could hardly extend beyond the point where the temperature becomes constant. In such a case the progression of the increase of temperature would be found very different in those places.

The climate of a place is influenced by a superabundance of woodland and humidity, and consequently by aridity and barrenness; the former causing cold, and the latter increasing the warmth of the climate; further, by the radiation of heat from heated plateaus, the nature of the soil, the vicinity of glaciers, the accumulation of clouds, and so forth. Thus, for example, according to Boussingault, the mean temperature of the valley of *Magdalena* in the *Cordilleras*, situated at a height of more than 600 feet, is the same as at *Carthagena*, on the sea coast. On the other hand, geological peculiarities and proximity to active volcanos do not affect the climate. *Puracé*, *Pasto*, and *Cumbal*, which are commanded by active volcanos, have not a higher mean temperature than *Bogota*, *Santa Rosa de Osos*, and *Puramo de Hervé*, which repose on a perfectly unvolcanic soil.

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* Poggen. Ann. vol. xvi. p. 262. See also Reich. p. 20?.

This subject is treated at greater length in Chapters XVII, and XVIII.

CHAP. XVI.—*The unequal power of conducting heat, possessed by different rocks, causes differences in the progression of the increase of temperature towards the centre of the earth.*

This seems to be particularly striking with respect to metalliferous lodes. Fox[*] observed in a mine in *Cornwall*, that thermometers sunk into a metalliferous vein, stood in general 2°.25 higher than when placed in holes bored in the rock, particularly in granite. Tin veins usually shewed themselves colder than those which yielded copper. The tin mines of the *Sauberg* at *Ehrenfriedersdorf*[†] also shew a remarkably low temperature; indeed, it is a prevailing opinion there that stanniferous mountains are colder than others. At *Ehrenfriedersdorf*, the correctness of this opinion could never be questioned, because, in many parts of the mine, ice is at all seasons of the year to be found in considerable quantities. The low temperature of *Heinrichssohle*, in the Altenberg district in the *Erzgebirge*, was also supposed to be ascribable to the rocks of tinstone.[‡] It may, however, be questioned whether this phenomenon be not a consequence of the dislocated state of the rock, caused by the peculiar method in which it is worked.

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[*] Annal. de Chim. et de Phys. vol. xvi. p. 80. Fox remarks that at equal depths, granite and other hard rocks are, in general, of lower temperature than clay-slate and other soft and porous rocks.

[†] Reich. p. 87 and 107.

[‡] Ibid. p. 87 and 107.

Arago, with justice, considers this phenomenen of great importance, and remarks, that its further investigation would not only serve to prove the existence of a superior temperature in the interior of rocks and metalliferous veins, but would afford an additional objection to the hypothesis, supported by several philosophers, that the higher temperature in mines is the result of the action of the air upon substances, particularly the pyrites, on the walls of the galleries. To prove that the observation alluded to indicates a more important source of heat in the interior, he adds two mathematical formulæ from Fourier's Theorie de la Chaleur, from which it follows that, *cæteris paribus*, the temperature of the mass of a lode which is in contact with the atmosphere is greater, the more easily heat is conveyed into the interior of the mass. Cordier,[*] in his observations on mines, also mentions the great influence of a metalliferous zone on the subterranean temperature, owing to its superior conductibility of heat.

From the observations on the internal temperature of the earth, carried on in various mining establishments in the Prussian dominions, it has also been found, that the increase of temperature is, in general, much more rapid in coal than in metalliferous mines.[†] Von Dechen, the publisher of these observations, leaves it undecided whether this greater increase of heat is produced by the decomposition of the pyrites and coal, in which case it would not exist before the opening of the mines,[‡] or whether it is a consequence of the locality of the mine. If we take into consideration the difference of the facility with which heat is transmitted by coal and metals, we shall be inclined, agreeably to the observations communicated above, to look upon this as the real cause of the phenomenon. From observations in five coal-mines, the result was, that an increase of temperature of 2°.25 occurred in 123.3 feet, and from observations in two metalliferous mines, the depth corresponding to an increase of 2°.25 was 325.5 feet.

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[*] Mém. de l' Acad. Roy. de Sciences de Paris, vol. vii. p. 473.

[†] Poggend. Annal. vol. xxii. p. 497 and following.

[‡] D' Aubuisson (Journ. des mines. vol. xxi. Annal. de chim. et de phys. vol. xiii. p. 184. See on the other hand, Fox, in Gilb. Annal. vol. lxxvi. p. 427, 442, and 448.) found, in a mine near *Huelgoat* in *Bretagne*, the temperature in the upper works to be 6°8; but at the depth of 230 metres 15°8, and the water vitriolic. See Chap. III.

Supposing that the temperature in the upper station of the metalliferous mines, where observations on the increase of temperature were made, should be found 2°.92 higher than in the coal mines, whilst the temperature in the inferior station of both mines would be the same, the depth, corresponding to an increase of 2°.25, would be in the metalliferous mines also 123.3 feet.

It would be difficult to make a correction for this not unimportant influence on the increase of temperature, proceeding from the unequal power of conducting heat of different rocks.

CHAP. XVII.—*Can the increase of Temperature towards the interior of the Earth follow the same law in all parts of the Earth, departing from points all situated in the same level?*

According to Chap. VIII., the influence of the external temperature under the equator scarcely extends to the depth of twelve inches below the surface of the earth; whilst in the temperate zones it extends to a depth which has not yet been ascertained, but which in our latitudes, perhaps, amounts to sixty feet. Curves may, therefore, be supposed to be drawn from the equator to the poles; at the former reaching to within one foot of the surface; in our latitude lying at a depth of about sixty feet, and extending at the poles to a still greater depth, and thus describing the limit of the influence of the external temperature. A constant regular increase of temperature, dependent only on the interior temperature of the earth, can, therefore, only be supposed to commence below this limit; the crust of the earth above the line which marks the limit being exposed at the same time to the influence of the external temperature of the air, and of the interior heat of the earth. But under the equator the former influence disappears, and the increase of temperature depends solely upon the interior heat of the earth. In higher latitudes, on the other hand, both influences are found to operate. This we find by observations on the temperature of those springs which have their origin within this limit; namely, those springs observed by me, and cited in Chap. XVIII, No. 1, 8, 12, and 14, which shew yearly variations of temperature, but have yet a higher mean temperature than that of the place where they rise. In general, all thermal springs which shew yearly variations of temperature, may be considered as proofs of this assertion.

It must be remembered that the limit of the influence of the external temperature is absolute, but not so that of the internal. According as the depth below the surface be greater or less, so will it require a longer or shorter time after the minimum temperature of the air, before the temperature of the variable crusts of the earth will begin constantly to increase, until the temperature of the air reach its maximum. And according as the depth below the surface be greater or less, so will it require a longer or shorter time after that maximum, before the temperature will begin to decrease again, which will continue until the return of the former period. Thus, during the first half year, there will be a transmission of heat from the surface towards the interior; and during the second half-year, on the contrary, from the interior towards the surface. This oscillation during these two periods can, of course, only take place within the variable crust of the earth. Should ever the period from the minimum to the maximum increase to the double, the depth to which the external temperature penetrates would also increase almost to the double. That, in fact, the maximum and minimum temperatures of the air do not coincide with those of the soil; but that the yearly extremes of the latter occur, at the deepest points of the variable crust of the earth, according to the observations on the temperature of springs, about three months later than those of the former, is evidently a consequence of the slow transmission of the heat through the strata of earth and rocks. This circumstance, however, has no effect upon our considerations.

But how is it with the heat proceeding from the interior? Here we have no changes of the seasons. Here there is always an excess of heat in the interior, and a smaller quantity in the variable parts of the earth's crust. The direction of the communication of the heat must, therefore, always be the same; namely, from the interior towards the surface. In the part of the earth's crust, subject to variations of temperature, there is, consequently, a transmission of heat during one half of the year from below towards the surface; and during the other half, from the surface towards the centre of the earth; but from the interior it is emitted constantly from the centre towards the surface. That the latter is actually the case, is very clearly proved by the above-mentioned springs, which participate in the variations of temperature in the outer crust, but yet are thermal.

To find the law of the increase of temperature for a given place, entirely free from all external influence, the depth must always be first determined by observations, to which the immediate external influences penetrate. In Chap. VIII. we have already seen that this is a difficult problem.

If the external causes which have an influence on the temperature of the earth in a certain district, penetrate to a certain depth *t* feet; then, all other circumstances being the same, this depth would be the same in the mountains of that district as in the plains. Let us suppose the district only to be of such an extent, that *t* have the same value at every point of it; the limit of the external influences would then be parallel to the form of the surface, running horizontally beneath the plains, and rising under the mountains.

Supposing the decrease of temperature to depart from the centre of the earth, or from any spheroid concentric with the surface of the earth; it is immaterial what the temperature of the centre or of the nucleus bounded by that spheroid may be; this decrease of temperature, all other circumstances being the same, cannot proceed according to the same law towards all points of the earth's surface in concentric spheroids. For in that case there must be the same temperature at all points of the spheroid which forms the limits of the influence of the external temperature, whether under the equator, or under the poles. The decrease of temperature from the interior of the earth towards its surface must, therefore, follow different laws in different latitudes; that is to say, the temperature must increase more slowly in proportion to the depth, under the equator, than at the poles.

If we imagine curves drawn from the equator towards the poles, all points of which having the mean temperature at the equator, or 81°.50, they will touch the surface at the equator, but will diverge from it as the latitude becomes higher, and at the poles they will dip deepest into the earth.[*] If, for example, we draw such a line in the meridian of *Cornwall*, it will there dip to a depth of 144 fathoms ; for at this depth, according to Fox, the waters in the mines have the mean temperature of the equator. All these curves in the different meridians form a spheroid, which is somewhat more flattened than the surface of the earth. We may denominate these curves of equal temperature *subterranean Isothermal* or *chthonisothermal lines*, from χϑων, the earth. It is easy to be perceived, that these curves will bend sometimes upwards and sometimes downwards. Supposing the earth once to have had a higher temperature, equal throughout its surface, whether on mountains or in valleys; then the increase of temperature towards the centre, all other circumstances being the same, and supposing the atmosphere at any place to have had the same temperature at all heights, would, after the cooling down of the earth from the surface towards the centre, have followed the same progression from all points of its surface. But as the temperature of the air decreases in proportion to the height above the surface of the earth, the different points where the increase of temperature is the same, cannot, even within a small space, lie equally distant from the curved surface of the earth. If we apply this to an uneven portion of the surface of the globe, of so small an extent, that the decrease of temperature caused by the difference of latitude is not perceptible; then the curve ABC, Fig. 6, Plate II, parallel to the surface of the earth, will not have an equal temperature at every point. For, as the temperature of the air is lower at D than at E, the curve of equal temperatures must be different from ABC, and must pass through a point F, lying deeper, or further from the surface than B.[†]

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[*] If the mean temperature at the North Pole is, as Von Buch assumes, = 19°.85, and if the temperature there increases at the same rate as in the temperate zone, the curve of 81°.50 would fall at the North Pole to a depth of 3151 feet.

[†] In former observations on the temperature in mines, only the vertical depth in which a certain increase of temperature has been discovered, is given, without taking into consideration, that observations from E to A must

From this it follows, that a chthonisothermal line of any temperature, which was under a small district parallel to the surface, cannot continue its course under a neighbouring mountain, either parallel to the external configuration of the mountain, or in the continuation of its former direction, but must curve upwards as in the figure.

It might be for a moment supposed that the decrease of temperature from the base of a mountain to its summit, would follow the same law as that which takes place in the same elevation in the atmosphere. But in that case we could never find a higher temperature in the interior of a mountain than at its basis; which, however, is contradicted by the observations made in some mines situated in very high mountains in *America.*

The workmen in the mines of *Guanaxuato* in *Mexico* (21°15 north latitude) find themselves at a depth of 1607 feet in a temperature of 98°.15. This is about 15°.75 higher than the temperature of the equator at the level of the sea, and yet this point lies at a height of 4617 feet above the sea[*]. Boussingault[†] found the temperature of the soil near the *Hacienda del Rodeo*, at an elevation of 4128 feet, to be 68°; and Von Humboldt, at an elevation of about 11,200 feet above the sea, in the *Mina del Purgatorio*, in *Peru*, and in about the same latitude, found a temperature of 67°.28. The chthonisothermal line of 68°, therefore, rises 7072 feet between these localities.

The rising of the chthonisothermal lines in mountains of less considerable elevation is also proved in our quarter of the globe. In the bore at *Pregny*, near *Geneva*, (Chap. XIX.), a temperature of 63°.05 was found at a depth of 680 feet below the surface, and 511 feet above the level of the sea. De Saussure[‡] found the same temperature in the mine at *Bex*, 677 feet below the surface, and about 2300 feet above the sea. Thus the chthonisothermal line of 63°.05 rises about 1789 feet between

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give a very different result from those made between D and B. This point was first mentioned in Von Dechen's thermometrical observations in various *Prussian* mines.

[*] Von Humboldt in Gilbert's Annal. lxxvi. p. 450.

[†] Annal. de Chim. and de Phys., vol. liii. p. 245.

[‡] Voyages dans les Alpes, § 1088.

Geneva and *Bex.* *London*, situated in latitude 51°.40′ north, lies 14 feet above the sea, and the mean temperature of the soil is 51°.01. The eighth gallery in the *Churprinz-Friedrich-August, Erbstolle*, near *Freyberg*, in nearly the same latitude, (50°.55′), lies 41 feet above the sea, and 926 feet below the surface, and has a mean temperature, according to the average of three years' observations, of 60°.28[*]. Between these two points, therefore, which are situated in nearly the same latitude and elevation above the sea, there is a difference of temperature of 9°.27. The point in this *Freyberg* mine, at which the temperature of the earth is the same as that of the soil at *London*, was found to be 731 feet above the sea. So that a chthonisothermal line of 51°.01, supposed to be drawn from *London* to the *Erzgebirge*, would, at the former of these places, almost reach the level of the sea, but in the latter it would rise to a height of 731 feet above it.

Even many springs (Chap. XVIII) shew that the chthonisothermal lines suffer an elevation in passing under elevated parts of the earth's surface. Thus No. 1 and No. 8 shew nearly the same temperature. And as the latter lies 134 feet higher than the former, the chthonisothermal of 51°.35 must there rise 134 feet within a distance of about two leagues.

The rising of the chthonisothermal lines under mountains is, therefore, a very general phenomenon. Their inclination to the horizon is determined by the inclination of the side of the mountain, by the decrease of the temperature of the air or of the surface of the earth with the increase of elevation, and by the increase of temperature towards the centre of the earth. Let AB, Pl. II, Fig. 7, be the sloping side of a mountain, and let the mean temperature at A and D $= t°$, and at B $= t° - 1°$: then BD is the depth in which the temperature increases 1°, and CB = AE the height in which it decreases 1°; we have therefore:

$$\operatorname{tang} x = \operatorname{tang} \mathrm{BAC}.\frac{\mathrm{DC}}{\mathrm{BC}}.$$

$$= \operatorname{tang} \mathrm{BAC}.\frac{\mathrm{BC} - \mathrm{BD}}{\mathrm{BC}}.$$

When the depth below the surface, in which the temperature increases a certain number of degrees, is required, it may be measured either in a direction perpendicular to the horizon or to the surface of the earth at that place. In an horizontal plain these two directions become identical, and therefore it is unnecessary to make any distinction. This is not the case in mountains. A simple consideration shews, however, that the direction in which the increase of temperature in mountains must be measured is perpendicular to the slope of the hill, and not perpendicular to the horizon. If we suppose, for example, that a mountain forms a perpendicular precipice BC, Fig. 7, it is clear that the increase of temperature, caused by heat in the interior of the earth, cannot proceed in a vertical direction; for BC is a part of the earth's surface, and the surface is under the immediate influence of the temperature of the air. From B to C, therefore, there can be only such an increase of temperature as takes place in the same direction in the atmosphere [*].

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[*] Reich., p. 47.

That which is so evident in a mountain having a perpendicular side must also take place in mountains the slope of which is more or less inclined to the horizon. The increase of temperature in mountains can, therefore, only be measured in a direction perpendicular to their surface. So that in a conical mountain ABD, Pl. II, Fig. 8, we can only measure the increase of temperature from the exterior to any point F in the interior of the mountain, in the direction EF or GF. But since the increase of temperature towards the centre of the earth has hitherto always been measured in a direction perpendicular to the horizon, it remains to be determined in what ratio are the lines EF and BF.

Let the mean temperature at $B = t^\circ$, at $F = t^\circ + 1^\circ$, at $E = t^\circ + x^\circ$; and let the angle $B\,A\,C = a$; let the temperature be supposed to increase 1°, in every n feet, in a direction perpendicular to the slope of the mountain; and suppose the temperature of the atmosphere, or of the surface of the earth, to decrease 1° R. (= 2°.25 F.) for every additional m feet in height, then we shall have

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[*] If the increase of temperature in mountains proceeded in a direction perpendicular to the horizon, and at the rate of 2°.25 in every 115 feet, we should have an increase of temperature of 191°.25 from the summit to the foot of the celebrated tremendous precipice of *Caravalleda*, which has an almost perpendicular height of 9800 feet; so that at the foot there would be a temperature of 248°, as Von Humboldt found 56°.75 at the top at a time when the actual temperature in the *Guayra*, on the sea coast, was only 81°.50. See Gilbert's Annal., vol xxiv. p. 25.

$$\mathrm{B\,H} = m\,x$$

$$\mathrm{E\,F} = (t^{\circ} + 1^{\circ} - t^{\circ} - x^{\circ})\,n = (1 - x)\,n$$

but further,

$$\mathrm{B\,E} = \frac{\mathrm{B\,H}}{\sin a}$$

$$\mathrm{E\,F} = \mathrm{B\,E}\ \mathrm{cotang}\ a$$

$$= \mathrm{B\,H}\ \frac{\mathrm{cotang}\ a}{\sin a}$$

Substituting the above values of B H and E F, we find

$$(1 - x)\,n = m\,x\,\frac{\mathrm{cotang}\ a}{\sin a}$$

and from this we obtain

$$x = \frac{1}{\dfrac{m\ \mathrm{cotang}\ a}{n \sin a} + 1}$$

$$= \frac{n \sin a}{m\ \mathrm{cotang}\ a + n \sin a}$$

We have, therefore,

$$\mathrm{B\,H} = \frac{m\,n \sin a}{m\ \mathrm{cotang}\ a + n \sin a}$$

$$\mathrm{E\,F} = n - \frac{n^2 \sin a}{m\ \mathrm{cotang}\ a + n \sin a}$$

$$= \frac{m\,n\ \mathrm{cotang}\ a}{m\ \mathrm{cotang}\ a + n \sin a}$$

$$\mathrm{B\,F} = \frac{m\,n\ \mathrm{cotang}\ a}{\cos a\,(m\ \mathrm{cotang}\ a + n \sin a)}$$

Example: let

$a = 30^{\circ}\ n$
$n = 115$ feet
$m = 677$ feet

then we shall have

$x = 0^{\circ}.047$
$\mathrm{B\,H} = 31.65$ feet
$\mathrm{E\,F} = 109.62$ feet
$\mathrm{B\,F} = 126.62$ feet

Thus, if in plains an increase of temperature of 2°.25 be found at a depth of 115 feet, the same increase will not be found till a depth of 126.62 feet, in a vertical direction from the summit of a conical mountain, whose sides make an angle of 30° with the horizon. The relations will be the same, if from B (Pl. II. Fig. 9.) there extend a large plateau. The increase of temperature towards the interior of the earth will still advance more slowly in a vertical direction from B than in plains. But at a certain distance from B at K, the same increase of temperature will be found as in plains. In order to find the point K, we will suppose the temperature of the earth's crust at I to be y° higher than at B. If, further, the temperature at L be 1° higher than at K or at B, we shall have

$$\mathrm{IL} = (1 - y)\,n$$
$$\mathrm{KL} = n$$
$$\mathrm{KN} = m\,y$$

but since

$$\mathrm{IN} = \mathrm{IL}\sin a = (1 - y)\,n \sin a$$
$$\mathrm{LN} = \mathrm{IL}\cos a = (1 - y)\,n \cos a$$
$$\mathrm{KL} - \mathrm{LN} = \mathrm{KN}$$

by substituting the values of these lines, we shall have

$$n - (1 - y)\,n \cos a = m\,y$$

from which we get

$$y = \frac{n\,(1 - \cos a)}{m - n \cos a}$$

Substituting this value of y, we find

$$\mathrm{IL} = n - \frac{n^2\,(1 - \cos a)}{m - n \cos a} = \frac{m\,n - n^2}{m - n \cos a}$$

$$\mathrm{IN} = \frac{(m\,n - n^2) \sin a}{m - n \cos a}$$

$$\mathrm{KN} = \frac{m\,n\,(1 - \cos a)}{m - n \cos a}$$

If I B and N K be prolonged till they meet in O, we shall have

$$\mathrm{NO} = \mathrm{IN} \operatorname{tang} a = \frac{(m\,n - n^2) \sin a \operatorname{tang} a}{m - n \cos a}$$

$$\mathrm{OK} = \mathrm{NO} - \mathrm{KN}$$

$$= \frac{(m\,n - n^2) \sin a \operatorname{tang} a - m\,n\,(1 - \cos a)}{m - n \cos a}$$

$$\mathrm{BK} = \mathrm{OK} \operatorname{cotang} a$$

$$= \frac{(m\,n - n^2) \sin a - m\,n\,(1 - \cos a) \operatorname{cotang} a}{m - n \cos a}$$

and if we give to a, n, and m, the same values as in the above example, we shall find

$$y = 0°.0267$$
$$BK = 24.7 \text{ feet}$$

Under these circumstances, then, the increase of temperature towards the centre of the earth, would be found the same at K, at a distance of 247 feet from the point B, on the plateau B M, as in plains.

If the plateau B M be not horizontal, the value of B K may be easily deduced from the above formula. If B M rise, for example, at an angle of b from the horizon, the angle a is changed into $a - b$. If, on the contrary, B M fall at an angle of b, the angle a is changed into $a + b$. In the equation of B K, therefore, instead of a we must put, in the former case $a - b$, in the latter $a + b$.

The formulæ calculated above, enable us to describe the curves of the chthonisothermal lines in all mountains. It is evident, that they must form very irregular curves in mountains of irregular shapes. If a B C f (Pl. II. Fig. 10.) represent the vertical section of a mountain, the sides of which have an inclination of 30° and 72°, and having a horizontal plateau on its summit; and if, further, its height be 677 feet, so that, as it is above supposed, the temperature decrease 2°.25 from its foot to its summit, then the chthonisothermal line having the mean temperature of the foot of the mountain, will take the direction of the lines $a\,b\,c\,d\,e\,f$. The numbers 127, 115, and 249, represent the depths in feet, in which the temperature increases 2°.25, and 25 and 69 feet are the distances from B and C, at which the normal increase of temperature of 2°.25 in 115 feet will begin to prevail on the plateau. The dimensions in the figure are drawn exactly according to a scale.

Among all the observations which have hitherto been made for the sake of discovering, in a direct manner, the increase of temperature towards the interior of the earth, I only know one which has been made in an almost horizontal direction. It is that one which was made by Boussingault* in the metalliferous mountains of *Marmato*. This mountain is so steep that it may almost be called an enormous wall of porphyritic syenite. Several adits enter it at different heights. At the mouth of one of these adits, 4494 feet above the level of the sea, the mean temperature is 68°.0, and the temperature increases towards the interior of the mountain at the rate of 2°.25 in about 127.5 feet. Boussingault says *about*, because it is not uniform, but varies according to the thickness of the rock above the adit, or, which is the same thing, according to the form of the surface of the mountain. The miners work in an atmosphere, the temperature of which is nearly equal to that on the sea-coast.

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* Annal. de Chim. and de Phys., vol. liii., p. 240.

This fact, together with the remark, that the increase of temperature is regulated by the thickness of the rock above the adit, proves very clearly that the increase of temperature can only be measured perpendicularly to the surface of the earth. It is singular that the above result accords within a foot with the increase of temperature found in a vertical direction, in the *Erzgebirge* (see Chap. XIX). Supposing the side of the mountains of *Marmato* to be quite perpendicular, and that the temperature of the surface there decreases 2°.25 in 690 feet additional elevation (see Chap. XVIII.), the chthonisothermal line, having the mean temperature of the foot of the mountain, would then rise at an angle of 79°.32 with the horizon. This is, in general, about the maximum angle at which chthonisothermal lines can rise in mountains having a perpendicular ascent.

We must not leave the question undecided, whether the value of *n*, taken in a direction perpendicular to the surface of the earth, is always the same; all other circumstances remaining the same, whether in plains, in deep valleys, surrounded with high mountains, or in mountains. If the nucleus of the earth possesses a certain quantity of heat, which is radiated towards all points of its surface, it might be expected that this distribution of the heat would be unequal at different points, according as they were situated on lofty projections or in deep hollows; so that in the mountains, at the greatest distance from the centre of heat, the increase of temperature would proceed more slowly than under plains and valleys. But however probable it may be, that a certain portion of the heat which spreads to the more distant points, suffers a slower decrease from the centre towards the surface; yet it must, on the other hand, not be forgotte that the highest mountains are almost nothing in comparison with the diameter of the earth, and that where heat is forced to extend to greater distances from its source, a communication of heat takes place from all sides.

In the same manner as the chthonisothermal lines bend upwards in mountains, so will they, on the contrary, curve downwards under the sea, under lakes, and under glaciers.* The bottom of the sea, between a certain latitude in the frigid zones and the equator, has every where a lower temperature than it should have in consideration of its latitude alone. If, therefore, we draw a chthonisothermal line of t°, from a continent or an island, towards the bottom of the sea, since the temperature is there more or less under t°, the line must sink more or less below it. All other circumstances being the same, such a chthonisothermal line will suffer a greater depression, the nearer we approach the equator, and consequently the greater the value of t°. The same thing takes place under lakes and glaciers; and such a chthonisothermal line will sink deeper under these, *cæteris paribus*, the nearer they are situated to the equator, and the less their elevation above the sea. As, for example, according to Chap. XI., the temperature of the sea in its depth, even in the torrid zone, does not exceed 32°, the chthonisothermal of 81°.50 (the mean temperature on the equator) will sink to a depth of about 2500 feet beneath the bottom of the sea, provided the value of n be the same under the sea as under plains. Now if we take the highest mountain on the earth as the measure of the greatest depth of the sea, we should have two points under the equator of equal temperature, = 81°.50, the one touching the surface of the earth on terra firma, the other situated 26,500 feet below it. Similar differences will occur, though of smaller extent, between two points of equal temperature, one of which is situated on the banks of a lake, or near a glacier, the other beneath the bottom of the lake, or under the glacier.

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* That which holds good for glaciers is also applicable to accumulations of snow and ice in narrow valleys, only in a much less degree. See Von Buch in Gilbert's Annal., vol. xli., p. 2.

If the line DA, Pl. II. Fig. 11. represent a part of the surface of the earth on the sea-coast or on the banks of a lake; DAC the angle formed by the line AC, which runs along the bottom of the sea or lake to its deepest point C, and the line AD; and if, further, the mean temperature at D or A $= t^\circ$, at C $= t^\circ - p^\circ$, at B $= t^\circ - p^\circ + 1^\circ$, and consequently BC $= n$; we shall have

$$\mathrm{DAB} = \mathrm{DAC} - \mathrm{BAC}$$

and BC being perpendicular to AC we find

$$\sin \mathrm{BAC} = \frac{\mathrm{BC}}{\mathrm{AB}} = \frac{n}{\mathrm{AB}}$$

Thus, then, we find the inclination of the line AB, the chthonisothermal of t°, to the surface of the shore of the sea or lake.*

What will be the value of n, beneath the bottom of the sea, of lakes and of glaciers? As direct observations are here entirely wanting, we must be satisfied with determining, by general considerations, what will be the ratio of this value to that which has hitherto been found beneath that part of the earth's surface, which is not covered with water or ice.

Let us imagine to ourselves a point at the bottom of a lake, which, if the lake were not filled with water, but with the solid substance of the earth, would have a temperature t°, a temperature higher than 38°.75: then, if by any revolution of nature the solid matter above this point were suddenly to be removed and replaced by fresh water, this point would assume the temperature of the water. But this temperature would, after some time, change to that at which water assumes its greatest density, provided the temperature of the air at the surface had, during that time, been reduced so low, namely, to 38°.75 (Chap. XI). Under all these suppositions the said point would, therefore, have been reduced in temperature from t° to 38°.75. But since the difference of temperature between this point and n feet below it was originally $t^\circ + 2^\circ.25 - t^\circ = 2^\circ.25$, after it became covered with water this difference must have risen to $t^\circ + 2.^\circ25 - 38.^\circ75 = t^\circ - 36^\circ.50$. So that the higher the original value of t° was, the greater will be the difference after the filling up of the lake with water. Now, as in general, a difference of temperature of 2°.25 occurs in n feet, a greater difference of temperature than 2°.25 for the same depth cannot long exist. The cooling of the bottom of the lake, which at the moment of its filling with water is only superficial, will soon extend to a greater depth. But however deep this cooling may extend, yet there must be a point at which it becomes equal to nothing, or at least imperceptible. The increase of temperature beneath lakes, must, therefore, follow a more rapid progression, that is to say, the value of n must there be smaller than under the solid earth.

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* The deepest point of the bore at *Pregny* near *Geneva* is 680 feet below the surface of the earth, and is nearly on the same level with the bottom of the lake, near *Geneva*. The temperature of this point is 63°.05, that of the lake in its depths, and consequently, also of its bottom, is, according to De la Beche, 43°. If the temperature increases beneath the bottom of the lake at the same rate as in the bore, the point beneath the bottom of the lake, which has a temperature of 63°.05, must be situated 979 feet lower than the lowest point of the bore.

The same will be the case under seas and glaciers: under the sea, where it has a great depth, even in high latitudes, provided only that the temperature of the bottom of the sea do not reach that degree which it would have, were it completely filled up with the solid matter of the earth, and under glaciers also, only so long as the original mean temperature of the soil beneath them is not reduced to 32°. (Chap. IX). Under the deepest parts of the sea, the value of n will be considerably diminished; for, according to Chap. XI. if the depth of the sea, under the equator, for example, is equal to 6658 feet, the original temperature of the bottom of the sea would almost reach the boiling point of water. The reduction of this to 35°.96, the lowest temperature observed in the depths of the sea in low latitudes, would consequently require a very considerably more rapid increase of temperature towards the interior.

As the solid masses of our globe are very bad conductors of heat, the reduction of temperature consequent on the creation of a lake, or the filling up of an alpine valley with a glacier, must certainly ensue very slowly. We may obtain a measure for the time necessary for the progress of this reduction of temperature towards the interior, from the time which the temperature of the air, as it goes through the successive changes of the seasons, requires in the temperate zones, to penetrate to a certain depth into the crust of the earth. From the observations communicated in Chap. VIII. and XVIII, we find that at a depth of four to six feet the maxima and minima follow about a month later than in the air. In the shafts in which I am making thermometrical observations at various depths, the maxima and minima at a depth of 24 feet follow more than two months after those of the air. The observations in the *Erzgebirge* also shew, in general, that the maxima and minima take place later, the deeper the points of observation lie; however, there is no perfect regularity in their succession, which is, no doubt, caused by the distributing influence of the external temperature. The springs also, which shew the smallest variations in their temperature, reach their maxima and minima of temperature about three months after those of the air. But as the depths from which they rise are unknown, no positive conclusion can be drawn from them, as to the length of time necessary for the temperature of the air to penetrate to a certain depth. Thermometrical observations, extending to the greatest depths, to which the external influences penetrate, will, at a future time, furnish us with accurate data, from which to determine the time necessary for the spreading of heat into the interior of the earth. Till then we can always assume, that about three months must elapse before the temperature at the surface can reach a depth of about 60 feet. But this depth does not only depend on the conductibility of heat of the strata of rocks and earth; but also on the difference of temperature. For the greater the change of temperature which a point of the earth suffers, the more quickly will this change of temperature be communicated to the interior. The greater the reduction of temperature, therefore (to apply this to the example in question), which the bottom of a lake at the moment of its creation, or a valley which becomes filled up with a glacier, suffers; the more rapidly will the reduction of temperature, all other circumstances being the same, travel into the interior. This is perfectly analogous to the effect of latitude on the depth, to which the external temperature penetrates into the earth. In high latitudes, where the yearly variations of temperature are greater than in low ones, the external temperature also penetrates more rapidly than in low latitudes; because the point where the temperature first becomes constant, lies deeper in the former than in the latter. But that which is effected by a greater yearly variation of temperature, must also be effected by a greater instantaneous change of temperature.

Those seas and lakes, which have a great depth immediately near shore, will produce a depression of temperature on the shore, in consequence of the low temperature at their bottom. So that, if the mean temperature at a certain distance from the lake or sea, where its cooling influence can no longer be felt, be $t°$, the chthonisothermal of $t°$ will bend downwards on approaching the sea or lake. It would be interesting to discover the mean temperature of the earth on the shores of deep lakes or seas, and at various distances from them, in order by that means to ascertain to what distance their cooling influence extends, and to determine the consequent curves of the chthonisothermal lines. For want of such observations, continued as they should be for at least a year, I made some solitary observations on the temperature of the earth, at a depth of one foot, between the lakes of *Thun* and *Brienz*, on the 1st Sept. 1835, from which I obtained the following results:—

Lake of *Thun*, at its surface, close to the shore, near *Neuhaus*,		70.47
——— at the bottom, at a depth of 15 inches,		64.85
The *Aar*, where it falls into the lake,		66.0
The soil, temperature of the sand at the bottom of the lake, at the same spot,		58.10[*]
The soil, at another spot near the shore, where the lake is very shallow,		57.65 ⊙
		56.41 Sh.
	Mean	57.02
The soil, 230 paces from the lake towards *Unterseen*		61.92 ⊙
		57.20 Sh.
	Mean	59.56
——— half-way between the lake and *Unterseen*,		62.37 ⊙
		57.42 Sh.
	Mean	59.89
——— at *Unterseen*		61.47 ⊙
		58.55 Sh.
	Mean	60.01

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[*] The observations marked ⊙ were made in many places exposed to the rays of the sun; those marked Sh., on the other hand, were made in the shade on the north side of houses, walls, and so forth.

The soil near *Interlacken* towards the lake of *Brienz*, . .	63.05 ☉
	56.30 Sh.
Mean	59.67
———— 40 paces from the lake of *Brienz*, . . .	58.55 ☉
———— 8 paces from the lake of *Brienz*, . . .	55.40 Sh.
Mean	56.97

Thus, the temperature of the surface of the earth decreased 2°.92 from *Unterseen* towards the lake of *Thun*, as well as towards the lake of *Brienz*. But where the observations were made on the shore of the lakes, they become gradually deeper and deeper; and the deepest points in both lakes are at a great distance from the shore.

The following observations also prove the cooling influence of the bottoms of lakes on their banks.

Near *Därligen*, on the lake of *Thun*, . . .	57.87 ☉
At *Aeschi* on the *Niesen*, 930 feet above the lake of *Thun*.	57.20 ☉
	56.05 Sh.
Mean	56.62
At the lake of *Brienz*, near the *Giesbach*, and not far from the place where the lake is deepest, ...	53.37 ☉
1578 feet above the lake of *Brienz*, higher up than the waterfalls,	53.37 Sh.

Notwithstanding the immense difference of elevation between these two points, the temperature of the soil was the same. But I do not mean to maintain that there is no decrease of temperature between these two points. The mean temperature of the point situated 1578 feet above the lake of *Brienz* would certainly be found lower than that on the bank of the lake. At the time that I made those solitary observations, the temperature of the soil at a depth of 1 foot was above the mean; but more so at the higher point than near the lake, because the yearly maximum occurs later in higher regions than in valleys. Hence the equality of the temperatures observed at the two points, notwithstanding their considerable difference of elevation. However, it may almost with certainty be assumed, that, owing to the cooling influence of the bottom of the lake on the lower point, a smaller decrease of temperature would be found between these two points, than in places where no such cooling influences exist.

In the same manner, as lakes and seas cause a depression of temperature in their immediate environs, so do also the glaciers. To prove this, I also made some observations near the lower glacier of *Grindewald*, at a depth of 1 foot,[*] on the 26th August 1835, the results of which were as follows:—

Close to the glacier,	+36.50
On the banks of the glacier-stream, where the first vegetation, grass and alder-trees, begins,	46.62
100 paces from the same,	51.12
200 paces from the same, near the bridge,	51.12
460 paces from the same, beyond the bridge,	52.47
720 paces from the same, where the first potatoes are cultivated, and where the first maple and cherry trees stand,	51.8[†]
12.86 paces from the banks of the glacier-stream, the *Schwarze Lütschine*, near the bridge,	49.10[‡]
Only 8 paces from the same,	55.40
1788 paces from the glacier, near *Grindelwald*, where the first wheat is grown,	56.75

Quite close to the glacier the temperature of the soil is considerably modified by that of the air. Thus at the same spot, near the glacier, where the temperature of the soil, on the 26th August, the weather being cold and damp, was 46°.62, rose to 47°.97 on the 28th, when the weather had become fine and warmer, and on the 7th September, up to which time the days had continued warm, I found it as high as 49°.32. At *Grindelwald*, on the contrary, at a distance from the glacier, the temperature decreased from the 26th August to the 7th September, 32°.67, which was exactly correspondent to the advance of the season.

A chthonisothermal line of t°, at such a distance from the glacier that its cooling influence on the soil could no longer be felt, would, therefore, bend downwards on approaching the glacier.

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[*] The first observation only excepted, which, on account of the quantity of stones, could only be made at a depth of 6 inches.

[†] This somewhat lower temperature probably proceeds from a copious brook which runs down from the *Mettenberg*, at the very spot where the observation was made, with a temperature of 43°.81.

[‡] Here we see the cooling influence of the *Schwarze Lütschine* whose temperature is only 41°.00, but which disappears, however, as the succeeding observations shews, at a distance of 8 paces. On the 28th December Mr Ziegler found the temperature of the *Schwarze Lütschine*, under the ice, to be 32.22.

In order to obtain the elements from which to describe such a curve, I had a hole dug four feet deep, near the glacier of *Grindelwald*, and completed my preparations in the same manner as on the *Faulhorn* (Chap. XVIII.). Midway between the glacier and *Grindelwald*, at the foot of the *Mettenberg*, a second hole was dug, and in the garden of Mr Ziegler, clergyman at *Grindelwald*, I had a third made of equal depth. Mr Ziegler has had the kindness to undertake to observe the temperature of the water in the bottles buried in these three holes once every month, and has already communicated to me the first half-year's observations:—

		Near the Glacier.*	At the Foot of the Mettenberg.†	In the Garden.‡
1835.	Sept. 23.	39°.20.	47°.30.	52°.02.
	Oct. 27.	35°.82.	42°.12.	40°.85.
	Dec. 2.	33°.35.	37°.17.	41°.45.
	— 28.§	32°.67.	33°.57.	38°.30.
1836.	Mar. 1.	32°.22.	32°.67.	36°.95.
	April 4.	33°.12.	34°.47.	36°.07.
Half-yearly mean,		34°.39.	37°.88.	42°.26.

From these averages no conclusions can be drawn. But if the mean temperatures at these three points should fall on the

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* At the same spot where I found 46°.62 at a depth of one foot, on the 26th August 1836.

† This is the nearest point to the glacier where potatoes grow, and where maple and cherry trees are to be met with. At the same spot I found 51°.86, at the depth of one foot, on the 26th August.

‡ Near the spot where, on the same day, I observed 56°.75, at a depth of one foot.

§ It was impossible to observe at the end of January, on account of the great quantity of snow which covered the places of observation. Even on the 1st March, when the covering of snow had considerably decreased, it cost some pains to reach the places, especially the one near the glacier, where the snow lay about five feet deep. Mr Ziegler, however, does not think that it has caused a gap of any consequence; at least, if we compare the observations of 28th December and 1st March, there seems very good reason to believe, that the temperature could not have been lower between those two periods, as the water at the two former places of observation would otherwise have frozen and broken the bottles. However, if the mean temperature took place, as supposed above, on the 10th or 24th November, the minimum would have occurred on the 10th or 24th February; and in that case the temperature might perhaps have sunk to 32°, or even below 32°. But at this temperature the water in the bottles, being quite undisturbed, might have remained liquid.

10th of November, as was the case in my observations near *Bonn*, communicated in Chap. XVIII., we might by interpolation in the above observations obtain a mean for each of the three points, which would be as follows:—

Glacier.	Mettenberg.	Garden.
34°.90.	40°.28.	44°.84.*

But should they fall, according to my observation on the *Löwenburg*, on the 24th November, the interpolation would give for the mean temperatures—

At the Glacier.	Mettenberg.	In Garden.
33°.91.	38°.30.	42°.68.*

In the former case the increase of the mean temperature from the glacier to the foot of the *Mettenberg*, would be 37°.37; and from thence to *Grindelwald* 36°.56; in all 41°.94. In the latter case the increase would be, from the first to the second point, 4°.39, and from the second to the third, 4°.39, making in all 8°.78. Taking the mean of these two results, we find that the increase of temperature from the glacier to *Grindelwald*, a distance of about 4500 feet, is 9°.36. If, then, the increase of temperature towards the interior of the earth, advances 2°.25 in 115 feet at the glacier; the chthonisothermal of 43°.77, which, at *Grindelwald*, touches the surface, will pass at a depth of 478 feet under that point of the glacier, and the chthonisothermal of 39°29, which touches the surface at the foot of the *Mettenberg*, will dip 250 below the same point. As the second point of observation, at the foot of the *Mettenberg*, is situated about 1800 feet, and the third at *Grindelwald*, about 4500 feet distant from the glacier; and as, farther, the two first places are nearly on the same level, but *Grindelwald* 248 feet higher than the lower end of the glacier; those chthonisothermals must form an angle with the horizon of about 9°.10′ to 10°.27′.

These relations are illustrated in the diagram, Fig. 5, Plate 2, where *c* is the point of observation of the glacier, *d* the point of observation at the foot of the Mettenberg, *b* the point of observation in the Grindelwald, *ab* the horizontal line, Fig. *cdb* profile from the glacier of the Grindelwald, *ed* the chthonisothermal line of 39°.31 Fahr., and *fgb* the chthonisothermal line of 43°.77 Fahr.

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* Heer found the mean temperature of the air at *Matt*, in the canton *Glarus*, 2560 feet above the level of the sea, between August 1831 and August 1832, to be 43°.85. (Fröbel and Heer, Mittheilungen aus dem Gebiete der theoret. Erdkunde. Zurich. 1836. P. 492.) As *Grindelwald* lies 677 feet higher than *Matt*, and in the vicinity of the glacier, 42°.68 is probably nearer the truth than 44°.84·

From these observations it is evident, that the cooling influence of glaciers on the soil in their vicinity is not felt to any great distance; but that it is soon counteracted by the heat of the air. This accounts for the remark so often met with in descriptions of travels, that flowers and strawberries may be gathered within sight, nay in the immediate neighbourhood, of the glaciers.

The mean temperature at the glaciers is about the same as that of *Barnaul*, in Asiatic *Russia*, 53°, 20′ N. Lat.; that at the foot of the *Mettenberg* is about equal to that of *Moscow*, 55° 47′ N. Lat.; or to that of *Drontheim*, 63° 26′ N. Lat.; and at *Grindelwald* it is about the same as at *Königsberg* in 54° 42′ N. Lat.* Such various mean temperatures and such various climates are, therefore, crowded together in the vicinity of that glacier.

The observations and experiments which have formed the subject of this chapter, answer the question, whether the increase of temperature towards the centre of the earth follows the same law, departing from all points situated on the same level, in the negative. The experiments have also made us acquainted with the chthonisothermals, lines of equal temperature, which, rigorously speaking, only remain parallel with the surface in plains within a very limited space, but bend upwards under mountains, and suffer a depression under lakes, seas and glaciers; thus always forming a greater or less angle with the horizon. Besides this, they must be a function of the curvatures found by Von Humboldt in the isothermal lines; for just as little as these coincide with the latitudinal lines of the earth,† will also the points of equal temperature in the interior of the earth be situated at equal depths in the same latitudinal circle.

I will conclude this chapter with an application of the fact, that the chthonisothermal lines curve upwards in mountains, to the theory of the creation of those warm springs, which rise at the foot of high mountains. As an example, I will take the warm springs of the baths of *Leuk*, in the canton *Wallis*.

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* Kämtz Lehrbuch der Meteorologie, ii. 88.

† Schweigger's Journal, xxv. 261.

The highest point near the *Leukerbad* is the *Balm Horn* (11,415 feet above the level of the sea). Now if, according to Chap. XVIII, the mean temperature of the soil is 32° at a height of 6195 feet, and if the decrease of temperature at the surface is 2°.25 in every 677 feet additional height, the mean temperature at the summit of the *Balm Horn* must be + 14°.56. If, further, the angle which the steep sides of the *Balm Horn* form with the horizon is 45°, and this mountain be considered as a cone; we find from the above formula that the increase of temperature from the summit towards the interior, is equal to 2.°25 for every 145 feet. The temperature 32° must, therefore, be found in the interior of the mountain, at a depth of 1123 feet from its summit, or 10,202 feet above the surface of the sea, and water may be supposed to exist there in a liquid state. From there to the level of the *Baths of Leuk* (4275 feet above the sea,[*] the increase of temperature, supposing it to continue to advance 2°.25 in 145 feet, must amount to 125°.37. If, then, we imagine water to collect in the *Balm Horn* at an elevation of 10292 feet, and to flow down through clefts in the interior to the level of the *Baths of Leuk*, these waters will have become heated on their way to a temperature of 125°.37, and will appear at the surface with this temperature, which is equal to that of the hottest of the springs of *Leuk*. There is no contradiction in supposing liquid water to exist in the interior of a mountain under a covering of perpetual snow. We find spring-water in mines, even when the surface has been covered for months with snow. At *Bogoslowsk*, where the earth is covered with snow during the greater part of the year, the quantity of water in the mines is nevertheless no greater in summer and autumn than in winter, and only increases in spring, when the flood of the meteoric waters is considerably increased by the sudden melting of the snow, and the overflowing of the rivers.[†] At *Hudson's Bay*, where the mean temperature is far below the freezing point, springs flow through the winter, under a covering of ice and snow.* The supposed collection of water in the interior of the *Balm Horn* (which supposition is justified by the sinking of such enormous bodies of water as those of the *Dauben See* on the *Gemmi*, as well as by other phenomena, from which it is evident that the limestone has there suffered very considerable fissures in the interior) may be fed during the summer by the partial melting of the snow, for even on the summit of *Mont Blanc* it sometimes rains during the warm summer months, and the snow is there sometimes seen to begin to melt.

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[*] According to Michaelis, (p. 272.) 4402 feet.

[†] Kupffer in Poggendorff's Annal. vol. xv. p. 185.

These considerations prove the possibility of the creation of warm springs in the Alps, when water in the interior of the mountains comes down from a great height, and, after passing through the warmer strata of the earth, makes its appearance in the valleys below. Such warm springs may even rise in very considerable elevations, provided there be still higher mountains in the environs. I will only bring forward one example more: if water should penetrate from the above mentioned mines of *Guanaxuato* in *Mexico*, where the temperature is already as high as 98°.15 into the interior of the mountains, as low as the level of the sea, its temperature would then have risen to 189°.50. Thus, then, in the high mountains of *America*, boiling hot springs may be produced, merely by the infiltration of meteoric waters into the interior of the earth, at an elevation of 9000 to 10,000 feet, and their reappearance at the level of the sea. And as this height is more than 4000 feet below the limits of perennial snow, the infiltration of the meteoric waters may be supposed to continue uninterruptedly in winter as well as in summer. The existence of such thermal springs, therefore, does not require that the waters should be brought to the surface by hydrostatic pressure.

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* Captain James, 1631. See Von Buch in Poggend. Annal. xii. 405.

Experiments on the Development and Growth of the Fry of the Salmon from the exclusion of the Ovum to the Age of Seven Months. By Mr John Shaw, Drumlanrig, Dumfriesshire. Communicated by the Author.*

There are few subjects which have hitherto afforded more scope for speculative treatment than the natural history of the salmon. From its birth to its mature age its history is involved in a degree of obscurity which can only be removed by an accurate and long-continued series of experiments. The medium in which observations must necessarily be made, the migratory habits of the fish, and the proneness of scientific men to rear systems upon partial and insufficient data, rather than to acknowledge the want of that correct information upon which alone systems can securely rest, all tend to embarrass the question, and discourage the investigator. These are difficulties which alike beset the path of the learned and unlearned; but there is another difficulty which more particularly presses upon the latter, and that is, the scepticism with which his observations are generally regarded by scientific inquirers. This scepticism must obviously be met by increased industry and caution, and by an accumulation of evidence so conclusive as to overbalance the disadvantage of a defective scientific education. In the observations and experiments which form the subject of this paper, I have therefore endeavoured to proceed with that circumspection and exactness, necessary alike to convince the incredulous and to protect myself from the charge of crude observation.

It was objected to the former paper which I contributed to this Journal, that my observations being made upon ova taken from the bed of the Nith, there was not sufficient evidence that these were the ova of the salmon at all, the same streams being accessible to other fish. However well satisfied I myself might be of the accuracy of my observations, I felt that it was desirable to supply this link in the chain of evidence, and I have accordingly repeated my experiments from the moment when the ova are excluded, preserving the skins of the parent fish that no doubt may exist as to their offspring being the progeny of the true salmon. I have also at stated intervals preserved specimens of the young to exhibit their progress, and to vindicate the opinion advanced in my former paper, that the salmon fry do not migrate the same year they are hatched, but continue in the river for two years after their birth. I may at the same time state, that I still adhere to my former opinion that the parr is the young of the salmon, although the families of the salmon in my possession have not yet reached that age when their identity may be most distinctly determined.

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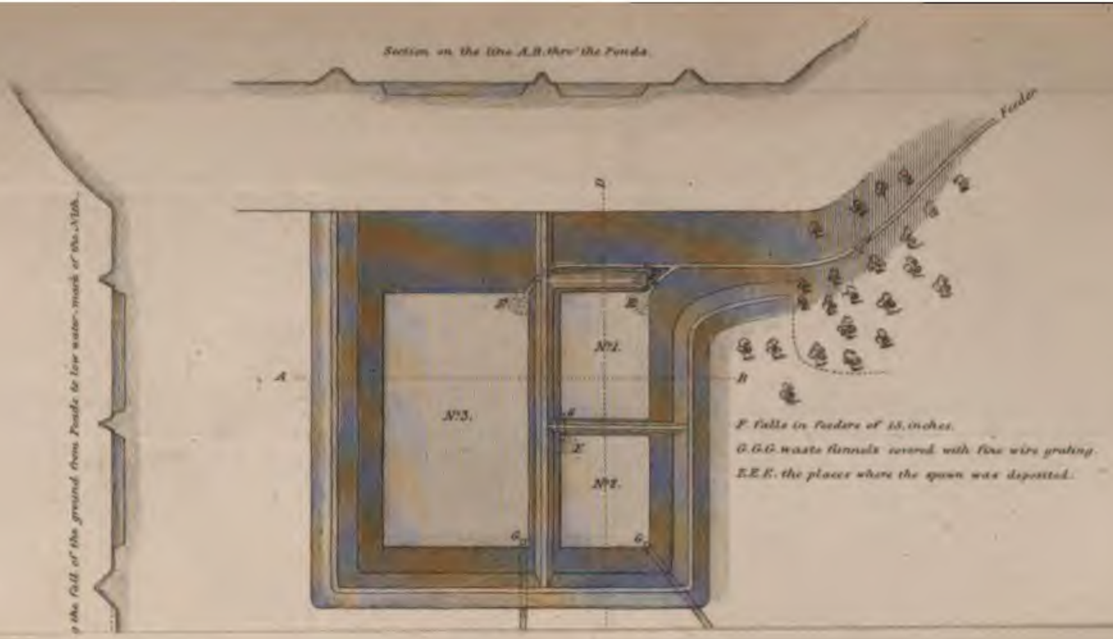
* Read before the Royal Society of Edinburgh on 18th December 1837.

Before proceeding to make the experiments about to be recorded, it was necessary to lay my experimental basins dry, not only for the purpose of removing the young salmon of last season's produce, but also to enable me to fit them up on such a principle as would exclude any possibility of confusion either from the overflowing of the ponds themselves, or from the flooding of the river Nith, on the banks of which they are situated. The plan on which these ponds are constructed is shewn in Plate III. Every precaution has been used not only to exclude error, but to place the young fry in circumstances as nearly resembling the state of nature as was consistent with their preservation.

The ponds, which are three in number, are two feet deep, and thickly embedded with gravel, while they are at the same time supplied with a small stream of spring-water in which the larvæ of insects abound. Pond No. 1 is 25 feet in length by 18 in breadth, and is fed by the stream which debouches into it at the fall F. Pond No. 2 is 22 feet in length by 18 in breadth, and is fed from pond No. 1 at G, where the communication is carefully grated with wire. Pond No. 3 is 50 feet in length by 30 in breadth, and is fed by the stream at F, having no communication with either of the other ponds. The waste water from pond No. 1 is conducted into pond No. 2 through a square wooden pipe covered at the mouth with a wire grating, the bars of which are about one-eighth of an inch apart. The waste water from pond No. 2 is conveyed under ground to the distance of 20 feet in a square wooden pipe grated in the same manner as the former. The waste water from pond No. 3 passes down a square wooden pipe 2 feet deep covered at the top with wire-gauze, and is conveyed under ground in a sma



Section on the line A.B. thro' the Ponds.

the fall of the ground from Ponds to low water-mark of the Nith.

F. Falls in Feeders of 15 inches.

G.G.G. waste funnels covered with fine wire grating.

E.E.E. the places where the spawn was deposited.

E. Mitchell, Sc.

red drain to the distance of 20 feet from the pond. The
er of the whole is then left to find its way to the river.

o prevent any communication arising from an accidental
flow of the ponds themselves, I have raised embankments
n the intersecting walks of 2 feet in height, so that the se-
l families of fish which the ponds contain can have no ac-
, direct or indirect, to each other. Where the rivulet is
ded for the purpose of supplying the several ponds, I have
ed an artificial fall in each stream, of a construction to pre-
t the fish from ascending one stream and descending an-
er. Finally, where the water discharges itself from the
ds, the channels are so secured by wire-grating that it is
npossible for the young fish to escape as for any other fish
ave access to them. The whole occupies an area of nearly
eet square.

My experimental basins being prepared, my next object
to secure the fish, the progeny of which were to form the
ect of experiment. With the view, therefore, of securing
salmon, male and female, while in the very act of propa-
ng their species, I provided myself with an iron hoop 5 feet
iameter, on which I fixed a net of a pretty large mesh, so
tructed as to form a bag 9 feet in length by 5 in width.
en attached the hoop and net to the end of a pole 9 feet
, thus forming a landing-net on a large scale. The weight
e net with its iron hoop being upwards of 7 lb., it instantly
to the bottom on being thrown into the water.

eing thus prepared with all the means of carrying my
eriment into practice, I proceeded to the river Nith on the
January 1837, and readily discovered a pair of adult sal-
engaged in depositing their spawn. They were in a si-
ion easily accessible, the water being of such a depth as to
it of my net being employed with certain success. Before
eeding to take the fish, I formed a small trench in the
gle by the edge of the stream, through which I directed a
l stream of water from the river 2 inches deep. At the
of this trench, I placed an earthenware basin of consider-
size, for the purpose of ultimately receiving the ova. I
, at one and the same instant, enclosed both the fish in the
, allowing them to find their way into the bag of the net

by the aid of the stream. In capturing these fish, I considered myself fortunate in securing them by one cast of the net, for, in conducting the experiment of artificial impregnation, it appears to me to be very desirable that the male should be taken, with the female of his own selection, at the very moment when they are mutually engaged in propagating their species. To take a female from one part of the stream and a male from another, might not give the same chance of a successful issue to the experiment. Having drawn the fish ashore, I placed the female, while still alive, in the trench, and pressed from her body a quantity of the ova. I then placed the male in the same situation, pressing from his body a quantity of the milt, which, passing down the stream, thoroughly impregnated the ova. I then transferred the spawn to the basin, and deposited it in a stream connected with a pond previously formed for its reception, which, however, I have not considered it necessary to represent in the accompanying plan. The temperature of this stream was 39°, of the river from which the salmon were taken 33°, and of the atmosphere 36°. The skins of the parent salmon are now in my possession.

On examining the ova on the 23d of February (fifty days after impregnation), I found the embryo fish distinctly visible to the naked eye, and even exhibiting some symptoms of vitality by moving feebly in the egg. The temperature of the stream was at this time 36°, and of the atmosphere 38°. On the 28th of April (114 days after impregnation) I found the young salmon excluded from the egg, which was not the case when I visited them on the previous day. The temperature of the stream was then 44°. On its first exclusion, the little fish has a very peculiar aspect. The head is large in proportion to the body, the whole fish measuring $\frac{5}{8}$ths of an inch in length, and of a pale blue or peach-blossom colour. The bag, which at this early age is appended to the upper part of the belly, presents a very singular appearance. It is of a conical shape, the base being attached to the fish. It measures about $\frac{2}{8}$ths of an inch in length, is of a beautiful transparent red colour, very much resembling a light red currant, and is easily distinguished at the bottom of the water, even when the fish itself can with difficulty be observed. A slightly indented fin or fringe resembling the tail of the tadpole extends from the dorsal and anal fins to the termination of the tail. The ova, which, for some time previous to being hatched, had been almost daily in my hand for inspection, did not appear to suffer at all from being handled. When I had occasion to inspect the ovum, I placed it in the hollow of my hand, covered with a few drops of water, where it frequently remained a considerable time without suffering any apparent injury. The embryo, however, while in this situation, shewed an increased degree of activity by repeatedly turning itself in the egg, an action probably produced by the increase of temperature arising from the warmth of the hand.

On the 24th of May (twenty-seven days after being hatched) the young fish had consumed the yolk, but in a few days afterwards the whole of this family, with the exception of one individual, was found dead at the bottom of the pond, a circumstance which has occurred more than once in the course of my experiments, arising, I apprehend, from a deposition of mud, the same result having been more than once produced, when the pond had not been sufficiently imbedded with gravel.

To shew the effect of increased temperature in hastening the development of the infant fish, I may relate an experiment which I made upon a few of the same ova, from which this family proceeded. On the 20th of April (106 days after impregnation) finding the ova alluded to unhatched, and the temperature of the stream being 41°, I took four of them and placed them in a tumbler of water, covering the bottom with fine gravel, in which I embedded the ova. I then suspended the tumbler from the top of my bed-room window, above which I placed a large earthen-ware jar, with a small spiggot inserted in its side, from which I easily directed a stream of pure spring water into the tumbler. The waste-water was carried out at the window along a wooden channel fitted up for the purpose. As there was no fire in the bed-room, and the window facing the north, the temperature did not range very high, 47° being the average, while the average temperature of the water in the tumbler was 45°. During the night, however, the temperature would be very considerably increased, and the consequence was, the young fish in the tumbler were hatched in thirty-six hours, while those remaining in the stream did not hatch till the 28th of April, a difference of nearly seven days. At this stage the little fish are so very transparent, that their vital organs are distinctly visible, and when placed immediately under the eye of the observer they present a very interesting appearance. The pectoral fin is continually in rapid motion, even when the fish itself is otherwise in a state of perfect repose. They also begin to manifest an increasing desire to escape observation, a principle wisely implanted in them for their better security, during so feeble and helpless a stage. On the 24th of May (thirty-nine days after their birth) the fish in the tumbler were completely divested of the yolk, and the characteristic bars of the parr had become visible. At this time they measured nearly one inch in length and appeared to be in perfect health, but fearing that after the yolk was consumed, I should be unable to supply them with appropriate food, I returned them to the pond from which I had taken them on the 20th of April, where they perished with the rest of the family.

This last experiment proves, that by placing the ova under a temporary stream of water in the house, the development of the young may be materially accelerated, while it also shews that they may be kept alive for a considerable time afterwards; at all events, until the yolk, which I presume to be their sole support at this period, is totally consumed. I mention this fact, because it has been stated by a recent author on the subject, that their life in a state of confinement could not be prolonged beyond a period of ten days.

The next experiment, the circumstances of which I have to relate, has been attended with more success than those which I had previously made. The process of taking the adult fish, and all the circumstances attending the impregnation, were entirely similar in this case to that already narrated.

That the pedigree of the young fish may not be called in question, I have preserved the skins of the parents. The weight of the male when taken was 16 lb., and of the female 8 lb.

The spawn was impregnated and deposited in the stream immediately below the fall pond No. 1. E, on the 27th of January 1837; the temperature of the water in the stream being 40°, and that of the water in the river 36°. On the 21st of March (54 days after impregnation) the embryo fish was visible to the naked eye. On the 7th of May (101 days after impregnation) the little fish had burst the shell, and were to be found amongst the shingle of the stream. The temperature of the water was at the time 43°, and of the atmosphere 45°. Fig. 1* is an accurate representation of the young fish ten days old, taken from the pond on the 16th of May. A considerable portion of the ovum is still attached to the abdomen. Fig. 2. is a specimen, forty-eight days old, taken from the pond on the 24th of June. Its symmetry is yet imperfectly developed. Fig. 3. represents the fish of two months' old, taken from the pond on the 7th of July. Its shape is now materially improved, and it presents in miniature the proportions of a mature fish. Fig. 4. is a specimen at the age of four months, taken from the pond on the 7th of September. The characteristic marks of the parr are now distinctly visible. The fish has attained a fine symmetry, and the features of the parents may almost be discovered in their child. Fig. 5. is a

Fig. 1.

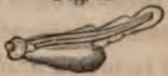


Fig. 2.

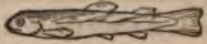


Fig. 3.

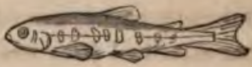


Fig. 4.

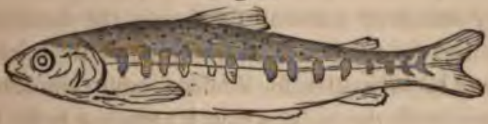
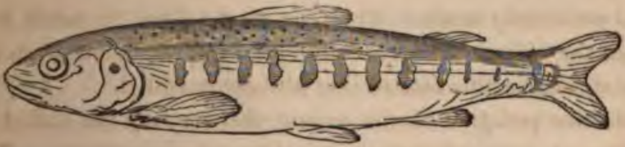


Fig. 5.



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* In reference to these illustrations, it may be stated, that the drawings having been made from the specimens after they had been for a considerable time preserved in spirits, they do not possess all the plumpness and elegance of contour which distinguish the fish in their natural state. For all general purposes, however, I believe they will be found sufficiently exact.

specimen of six months' old, taken from the pond on the 7th of November. Its shape indicates a considerable accession of strength, and its approximation to the features of the parent fish becomes more striking. The gill-cover, on examination, will be found exactly similar in shape to the full-grown salmon. On comparing it with the parr in the river, of a corresponding age, no marked difference can be observed. The whole of this family, as well as another family in pond No. 3, are in a state of perfect health, and feed freely on small worms, gentles, &c., with which I have continued to supply them during the summer.* As nearly as I can judge, the number of the two families amounts to about 100. Pond No. 2. is occupied with another family of the salmon, also produced by artificial impregnation. The history of this family will form the subject of another paper, after I have had an opportunity of verifying the experiment by repetition.

On comparing the parr taken from the river at a corresponding period with those taken from the pond, I found that they were uniformly of a darker colour, arising no doubt from the fish in the ponds having been reared in pure spring-water, while the parr in the river have existed ever since their birth in water of a more impure quality. The surface of their bodies has, therefore, naturally acquired a duskier tinge, a result perfectly well known to all practical anglers who have angled in the various rivers in this country. The water of the pond, in consequence of being supplied from a spring at no considerable distance, is of a more equable temperature throughout the year than that of the river. The temperature of the former during the winter is seldom below 40°, or in the summer above 60°, while the temperature of the river, which is much more under the influence of the atmosphere, is more variable, and reaches to greater extremes both of heat and cold. I apprehend, however, that the mean temperature may not materially differ; and, while I freely admit that the difference may have some influence on the progressive growth of the fish by which they are respectively inhabited, I still believe the final result will prove almost the same.

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* The circumstances attending the development and growth of the family in Pond No. 3 so exactly correspond with those of the preceding family, that their history would only be a repetition of the former. I may, however, state, that the individuals in Pond No. 3 are considerably larger than those in Pond No. 1, the difference at the age of six months amounting to an inch.

Before concluding this paper, it may not be altogether out of place to detail the particulars of a few simple experiments made to elucidate a very extraordinary phenomenon connected with this fish, namely, the fact of its changing colour under certain circumstances.* In the course of my visits to the experimental pond, which were as frequent as other duties would permit, I had often observed, that, while the little fish remained stationary in any particular part of the pond, they were always found to be of a colour corresponding to the colour of the bottom, and when they removed to any other part of a different colour, that, after resting on it for a few minutes, they gradually assumed a corresponding hue. Wishing, therefore, to prove the fact of this assimilation by actual experiment, I procured two large earthenware basins, one nearly white inside, the other nearly black. I then placed a living fish in each, while at the same time I kept up a constant supply of fresh water in them. The fishes were of their natural colour when first placed in the basins; but they had not remained there more than four minutes till each had gradually assumed a colour nearly approaching to that of the respective basins in which they were placed. I next took the fish out of the white basin and placed it in the black one, and the fish which was in the black basin I placed in the white, and the results were uniformly the same,—the fishes changing according to the colour of the surface over which they were placed. I next placed both fishes in one basin, when the contrast for a short time was exceedingly striking. With the view of ascertaining what effect the light had in producing this extraordinary change, I next allowed the fish to remain in the white basin so long as effectually to acquire the light tint, when I excluded the light from them altogether by covering the basin with a thick mat, and on removing it a few minutes afterwards, I found the fish were again changed to a dark colour, which gradually disappeared on exposure to the light. The change of colour is produced alike under a bright or cloudy sky. This singular phenomenon, with which I have only now become acquainted, adds another to the many beautiful provisions Nature has made for the safety and protection of her creatures. The cause, however, is a problem I make no pretensions to solve.

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* Since these observations on the changing colour of the fish were put in types, my attention has been directed to an interesting paper on the same subject in reference to the minnow, stickleback, and perch, contributed by Dr Stark to the Number of this Journal for October 1830.

To recur to the subject of the salmon's growth, I trust I have now succeeded, by a process of demonstration the most exact that could be attained, in establishing the fact, that the *young salmon does not proceed to the sea the same year in which it is hatched.* Although I may not yet have succeeded in convincing naturalists of the identity of the parr and the young salmon, it may yet be conceded that my researches have not been altogether unproductive of benefit, either in a scientific or economic point of view, if I have corrected an erroneous opinion as to the growth of the latter, entertained both by the legislator and the naturalist. The belief that the salmon migrates the same year it is hatched, has created an indiscriminate slaughter of that fish, at an age when it especially requires the protection of the Legislature. There is no fish in our rivers that takes the fly more readily, and every little tyro who can cast his angle on the stream, can reckon pretty confidently on killing eight or ten dozen per day. Where a salmon river, therefore, runs through a populous country, the destruction of the fry from this cause alone is incalculably great. It is true the Legislature has made provision for protecting the young salmon for one month, viz. during the brief period it remains in the river after assuming the migratory dress, but for the two first years of its existence it is at present entirely unprotected.

Before concluding, I may advert to the perplexing circumstance of the apparent maturity of the sexual organs of the male, and the immaturity of those of the female parr. In my former paper in this Journal I have stated as follows:—

> "That the female parr does not spawn is undeniable; and although the male parr of eighteen months old is to be found in the river, with the milt flowing from it in abundance, all the winter round till about the end of February, yet no instance has fallen under my observation of the roe in any female of the same age, or indeed of any age, having advanced to similar ma-

turity. The female parr may be found in the river in autumn, in nearly equal numbers to the male, but the roe found in it has not the most distant appearance of approaching to maturity. I have also taken it at times during the whole winter, when the weather was mild, and still the roe had no appearance of advancing; and even up to the period of their migration, it is to be found with the roe in the same immature state."

The apparent maturity of the male is a circumstance which cannot be reconciled with any of the existing theories regarding the parr; and it is also, I confess, a difficulty not easily reconciled with the opinion I entertain as to its being the young of the salmon. I trust that I shall not invalidate my statement of observed facts, if I venture to suggest a hypothesis upon which a speculative solution of the difficulty may be attempted. In the case of the hive-bee, we see a vast preponderance of males in the economy of the hive. The queen is productive to an extraordinary extent. She receives the conjugal embraces of all the males in the community, to enable her to perpetuate the race, and send forth infant colonies to compensate for the annual destruction of the species. The roe of the female salmon is prodigiously abundant; and when it is considered that she spawns in streams where the milt of the male may be often carried down without properly performing the function of impregnation; when it is considered, besides, that the male parr is found upon these streams with the milt flowing from him in abundance, have we not some ground for suspecting that the female salmon, like the queen-bee, has the aid of a plurality of males in propagating her species? I know it may be said, that these parr attend the female for a more exceptionable purpose, viz. that of devouring the ova which descend with the stream, and there is no doubt they do so to a very considerable extent; still, it is not beyond the bounds of possibility, that the gratification of two appetites may be subserved at the same time, and that they may be as instrumental in perpetuating their species as they obviously are in destroying it.

To conclude, I feel that my paper will be liable to many objections in a scientific point of view; but, as I make no pretension to scientific attainment, I am entitled to the indulgence of scientific men. I claim only to be considered a careful practical observer, and an honest inquirer after truth, possessing facilities of observation peculiarly advantageous, and a disposition to take advantage of these facilities to advance the knowledge of this interesting subject. For a number of years I have resided in the immediate neighbourhood of some of the best spawning streams of the river Nith, and during the spawning season I have devoted more attention to the operations of the salmon than most men under any circumstances have an opportunity of doing. Finally, I may state, that I shall continue to take specimens of the young salmon from my ponds, at stated periods, until they assume their migratory dress, when I shall embody my observations in another paper on the subject.

1. *On the Erratic Blocks of the Jura.* By M. Agassiz.—2. *On the Production of Crystals of Insoluble Substances by Artificial Means.* By M. Gaudin.—3. *On the Operation of the Earths in the process of Vegetation.*—4. *On the Algæ which communicated a Red Colour to the Waters of some Salt Marshes.* By M. F. Dunal.

41

1. *On the Erratic Blocks of the Jura.* By M. Agassiz.

Last year I spent several months in the neighbourhood of the Alps, for the purpose of studying the glaciers, and of examining the observations of MM. Venetz and Charpentier on the great *moraines*, which are found at distances more or less considerable from the existing glaciers; and I have convinced myself of the accuracy of the facts adduced by these observers.

I have not been less struck by the polished appearance presented by the rocks on which the glaciers have moved, an appearance which is still to be remarked in all the valleys, whose flanks are covered by ancient *moraines*, at whatever distance they may occur from the existing glaciers. Thus the flanks of the valley of the Rhone are entirely polished, even to the banks of the lake of Geneva, more than a day's journey from the glaciers, wherever the rock has been sufficiently hard to withstand the action of the weather. At the sight of this phenomenon, evidently produced by the glaciers which extended to the plains of Switzerland, and which, when they retired, left on their edges these concentric dikes of rounded blocks termed *moraines*, I remembered that the northern slope of the Jura, which faces the Alps, also presents polished surfaces, known to us by the name of *laves*, and to which I had hitherto paid but little attention. On my return to Neufchâtel, I hastened to examine more attentively these polished surfaces, and I have ascertained that they are completely independent of the stratification of the beds, and of the direction of the chain of the Jura; that they extend along the whole surface, following its undulations, passing equally over the *Neocomian* and the Jurassic formations, and presenting a polish as uninterrupted as the surface of a mirror, wherever the rock has been recently exposed. These surfaces are sometimes even, sometimes undulated, often traversed by furrows more or less deep and sinuous, but which never occur in the direction of the slope of the mountain. On the contrary, these furrows are oblique and longitudinal; in short, they have a direction which at once excludes the idea of a current of water having been the cause of the erosions. Besides, we remark, on the surfaces that are well preserved, fine lines, similar to the markings that would be produced by a diamond point on glass; and which follow, in general, the direction of the oblique furrows. To any one who has seen the Alps, it is evident that it is the ice which has produced this polishing. It is over an extent of more than twenty leagues to the east and west of Neuchâtel, that the northern slope of the Jura presents this aspect, that is wherever it has been examined in this point of view, and nevertheless these facts have remained unknown to the present time.

As we find the erratic blocks of the Jura reposing on these polished surfaces, the question naturally presents itself, Whether, as M. Charpentier had admitted, the glaciers had really extended to the Jura range, pushing before them blocks of alpine rocks, and polishing the surface on which they moved? A constant fact opposes this idea, an idea, indeed, which M. Charpentier has already abandoned, viz. that the erratic blocks of the Jura are angular, while the blocks of the *moraines*, constantly pushed by the movements of the glaciers and rubbed against one another, are always rounded. This is also the case with blocks of the most recent *moraines*, with those which accompany existing glaciers; now, if the erratic blocks of the Jura had been pushed by great glaciers to a distance so considerable as that of the Jura, they ought to be much more rounded than those of the *moraines*. The same ought to have been the case if the erratic blocks had been transported by currents of water, whatever may have been their depth or rapidity. By this last hypothesis, it is impossible to explain how the lakes situated between the Alps and the Jura were not completely filled up, especially as we can demonstrate that they existed prior to the transport of the blocks. Very recently it has been attempted to reconcile these various phenomena by admitting, that the transport of the angular blocks was effected on floats of ice moved by currents of water; but facts which I have observed on the Jura, and which had not been previously noticed, oppose this explanation. The erratic blocks of the Jura do not repose immediately on the polished surfaces; wherever the rolled pebbles which accompany the large blocks have not been disturbed by subsequent causes, we find that they form a bed of some inches sometimes of several feet in thickness, on which the angular blocks repose. These pebbles also are much rounded, even polished, and are so arranged that the larger are at the surface, and that the smaller, which often pass into a fine sand, are at the bottom, immediately on the polished surface. This order of superposition, which is constant, is completely opposed to the idea of transport by currents; for, in this latter case, the order of superposition of the rounded pebbles would be the reverse. The presence of a fine sand at the surface of the polished rocks also proves, that no powerful cause has acted on the surface of the Jura since the period of the transport of these alpine rocks; and it is without doubt to the pressure of this sand on the polished surfaces that we are to attribute the fine lines engraved on them, and which would not have existed if that sand had not been moved by a current of water; for neither our torrents, nor the water of our lakes when violently agitated, produce any thing at all analogous on the *same* rocks, even when they are charged with sand. I do not doubt that most of the phenomena attributed to great diluvial currents, and especially those recently described by M. Selfstroem, have been produced by ice.

It would be of great importance to ascertain if, towards the poles, and in general where erratic blocks exist, the surface which supports them is polished as in the Jura; if every where the large erratic blocks at a distance from the chains of mountains whence they were derived are angular like those of the Jura, and if, finally, they repose every where on a bed of rounded pebbles which are smaller the nearer they are to the polished surfaces. If the academy could obtain information on this subject from M. Dumont d'Urville, regarding the countries which he is about to visit, a great step would be made towards the determination of one of the most important questions in modern geology.

2. *On the Production of Crystals of Insoluble Substances by Artificial Means.* By M. Gaudin.

I have now the honour of submitting to the examination of the academy (French Academy of Sciences) various specimens of insoluble salts, in the form of very perfect microscopic crystals, obtained by a process which I deem applicable to all these substances, and capable of supplying them in crystals of all dimensions. The process consists in placing certain solutions in an artificial atmosphere; as, for example, by placing under the same receiver a cup containing moist carbonate of ammonia, and a glass filled with a weak solution of any salt of lime, barytes, lead, &c. At the end of some hours, there is deposited upon the sides of the glass, crystals of a very pure carbonate, of these several bases, as may be seen in the specimens which accompany this communication.

With regard to crystals composed of elements which are scarcely or not at all volatile, other arrangements must be had recourse to. Thus, I have obtained the sulphate of barytes, for example, by putting under the same receiver a flask of fuming hydro-chloric acid, and a glass containing water, sulphate of lime, and carbonate of barytes.

Solutions of a salt of pure lime generally afford crystals under the form of primitive rhombohedrons, or the principal modifications of that form; whilst the solutions of aragonite simultaneously give crystals in the form of Iceland spar, and in the form of carbonate of barytes; and, what is very singular, a solution of common chloride of calcium, apparently free from barytes and strontia, has afforded, on the same glass-rod, (which I herewith transmit), on the one side, the form almost exclusively of carbonate of barytes, and on the other, the form of Iceland spar, a result which I did not at all expect, since the glass-rod in question was marked with micrometric divisions, on which I expected to see rhomboids which would be capable of doubling the traces made by the diamond.

The carbonate of barytes supplies crystals which are altogether peculiar; they assume various fantastic forms and arrangements, and look more like a plant than a mineral salt.

I have for some time been engaged in preparing *symmetrical* crystals, that is to say, such as have no attachment or adherence, and formed, consequently, in a liquid or gaseous medium. With this object I have carefully studied the phenomena presented by the snow during the last winter. Since that time I have obtained crystals of sulphate of tin, similar to those we find presented by snow, and which were formed in a whirlwind of the vapour of sulphur. I have also recognised in the white marble of the Pyrennees microscopic crystals of silica. These two kinds of crystals, of which I send specimens, are, as will be seen, beautifully perfect, and those of the silica will, I trust, one day help us to elucidate the origin of primitive limestones. I also transmit crystals of carbonate of lime and carbonate of barytes, which are likewise symmetrical: these I obtained by pouring into a boiling solution of carbonate of ammonia a solution of a salt of lime, or of barytes, and then agitating the mixture.

I now produce all these crystallizations by pouring into a tube the length of the finger a saline solution, and placing in the upper part of it, before corking, a small portion of cotton moistened with the substance intended to form the artificial atmosphere. Finally, to render these and all other microscopic observations less fatiguing, I have thought of adapting a lens of a short focus to a stopper pierced with a hole, which receives the tube, and in which it is made to glide at pleasure. Diametrically opposed to the lens is a hollow conical mirror, which, by its upper portion, reflects the light upon that part of the tube we desire to examine. In this way we may, with the elbows on the table, readily examine much in a short time, and for a long time, too, without the least fatigue. I conceive that this is by much the most simple and useful microscope.

As I am not near the termination of my labours on crystallization, I have thought it right, in the mean time, to publish a few of the results I have obtained. I purpose shortly to present to the Academy additional new crystals made in a similar manner, and at the same time a microscope made on the principles to which I have adverted.

3. *On the Operation of the Earths in the Process of Vegetation.*

The following is an abstract of a paper on this subject, lately communicated to the French Academy of Sciences by M. Pelletier. Resting his opinion on the observations of agriculturists and chemists, and more particularly on the analyses of various soils made by Chaptal, Davy, and himself, the author admits that a fertile soil must be formed of silica, alumina, and lime; that the fertility diminishes as one of these three earths predominates; and that it is almost null when the mixture presents only the properties of one of them. But why, and how is this mixture of three earths, with the addition of oxide of iron or magnesia, a necessary condition of fertility? "This question," says M. Pelletier, "has not yet been satisfactorily answered. The physical constitution of the soils, their hygroscopic properties, their power of being more or less strongly heated by the solar rays, are circumstances to which a certain influence may reasonably be attributed, but which, nevertheless, appear to be only secondary causes. It seems, on the contrary, evident, that the mixture of the different earths which compose the soil, acts on vegetation by an electro-chemical force, whose influence has been recognised in other circumstances, but which has not hitherto been pointed out as connected with the subject now under consideration."

M. Pelletier remarks, that in a fertile vegetable soil the silica, lime, and alumina, must exist in a state of simple mixture; that if these substances were combined the soil would be sterile; and that, in a mixture of these three earths, the fertility would cease, if the combination were to be effected instantaneously. "For," says he, "in a mixture of silica, alumina, and lime, there exists a force which must tend to combine these substances; the silica and alumina are electro-negative bodies in respect to the lime, and in their presence the lime must acquire the opposite electrical condition. Thus, according as exterior movements, which are foreign causes, place the molecules at a greater or less distance, and group them in different manners, electrical piles are established, the degrees of tension will vary, discharges will take place, and the soil will be as it were animated. The electric fluid which pervades it will excite the radicular stomata, and the absorption of the fluids proper to the nourishment of the vegetable will take place; and the radicular fibres impregnated by humidity will become charged conductors for transmitting electricity to the plant, an electricity which is certainly as necessary to life as light and heat.

M. Pelletier afterwards considers certain practical operations in agriculture for improving soils, such as the mixture of beds of earth of different kinds, the use of marl, the exposure of marls to the air, tillage, and the use of lime; and he endeavours to deduce from his theory an explanation of the useful effects produced by these operations. He then examines why, at great depths, where he thinks that the oxygen of the air and the carbonic acid cannot penetrate, the radicles of the old trees can find carbonic acid, which, when absorbed, furnishes the carbon necessary for the nourishment of the vegetable. He admits, seeing the tendency that silica and alumina have for combining with lime, that there is a reaction of these two earths on the calcareous carbonate, a combination, a formation of a silicate, and a disengagement of carbonic acid. Thus, then, according to M. Pelletier, at certain depths, and under influences hitherto but little known, the silica would decompose the carbonate of lime, while at the surface of the earth, and under the influence of external agents, the silicates would be decomposed by carbonic acid.

2. *On the Algæ which communicate a red colour to the waters of some salt marshes, by M. F. Dunal.*

We often perceive in the reservoirs of salt-works, termed *tables*, water of a beautiful rose colour with a violet reflection, or water having a ferruginous orange-red tint, at the edges of which we observe a scum of the same colour. Water thus coloured is very dense (25 to 26 degrees of Baumé), and is just at the point of depositing crystallized marine salt; but this phenomenon is by no means so common as it is believed to be, and, to use the expression of the salt-makers, it only occurs in old water. Very often the *tables* crystallize without our perceiving in the water any trace of red matter. Thousands of quintals of salt have been collected this year in the salt-works of Bagnas de Villeneuve, and at that of Peccais, and scarcely any coloured salt has been met with. The red colour of salt-marshes had been attributed to the presence of a small *branchiopode* the *Artemia salina;* but M. Dumal has visited several salt-works where that crustaceous animal existed in innumerable quantities, and where the water, nevertheless, remained limpid and colourless. The *Artemia salina* observed in these waters was not at all red; the young individuals had a greyish colour, and those more advanced in age a rose tint, approaching the colour of rust. When the water is concentrated by evaporation, the crustaceous animal acquires a red colour, but the water itself is not at all tinged. We cannot attribute the red colour of the water of salt-works to the dead remains of the Artemia salina; for, at the salt-works of Bagnas and of Peccais, MM. Dunal and Legrand observed a considerable quantity of the dead animals half decomposed, which had a milky appearance.

Not being able to assign the presence of the *Artemia salina* as the cause of the coloration of the water, M. Dunal began to investigate the phenomena with attention. On taking some water from the upper part of a pool, which seemed filled with a liquid of a beautiful rose colour, or rose tinged with a violet reflection, M. Dunal only obtained a colourless liquid; but, when he plunged his vessel to the bottom, he brought up some coloured matter. This substance, when submitted to the microscope, presented to M. Dunal numerous spherical globules, which were extremely small and hyaline, and seemed to be a true *Protococcus,* to which he has given the name of *Salinus.* This small plant is developed at the bottom of the water, and its beautiful rose or violet colour is reflected through the whole of the liquid which covers it. In other reservoirs where he did not find the *Protococcus* *salinus*, M. Dunal discovered another substance of a deep orange-red colour, which appeared at the surface of the water. On being submitted to a magnifying power of 200 times the diameter, this substance exhibited a union of numerous individuals of a species of the genus *Hæmatococcus*, one of the simplest of the family of the Algæ, and which is characterized by its red *seminules* or globules. It is worthy of remark that it is another species of the same genus, the *Hæmatococcus Noltii*, which gives the colour to the peat-bogs of Schleswig. The *cellules* of the *Hæmatococcus* observed by M. Dunal, and which he names *Salinus*, are spherical or elliptical, at first of an orange-red, and afterwards of a ferruginous colour. In the laminæ of crystallized salt, M. Dunal observed long reddish threads. These were produced by the *Hæmatococcus salinus* imprisoned in the crystals of salt; and these crystals being dissolved, the plant is reproduced in a state of perfect preservation. In the middle of the salt-work of Bagnas, M. Dunal has sometimes seen floating a red substance, which assumes the elongated form of a mass of Confervæ. The *Hæmatococcus salinus* was at that place mixed with another rudimentary alga, which is merely a simple hyaline tube, without ramification or articulation, terminated by a point, and perfectly empty. This is a species of *Protonema*, to which M. Dunal gives the name of *Salina*. Although this botanist mentions a *Protococcus salinus* and a *Hæmatococcus salinus*, yet he thinks that these two pretended species, which, according to the divisions generally adopted, it is necessary to range under two different genera, are one and the same plant, which, when young, is a *Protococcus*, and when more developed is a *Hæmatococcus*.

The salt assumes the tint of the different vegetables it encloses; it is orange-red, or of the colour of rust, when it contains the Hæmatococcus, and of a beautiful rose-violet tinge, when it includes the Protococcus. A delightful violet odour is exhaled by these coloured salts, and is retained for a year, when they are heaped up in prismatic masses termed *Camelles*. The colouring matter formed by the *Hæmatococcus*, full of globules of an orange-red tint, stains the hands strongly. M. Dunal gives no details regarding the *Artemia salina*, as M. Andouin has already announced that he is investigating that *branchiopode*.

Account of a Visit to the Falls on the Shiráwatí River in Canara. By the Rev. Dr Wilson of the General Assembly's Mission, and Dr Smyttan of the Medical Board, Bombay. Communicated by Dr Wilson to the Rev. Dr Brunton, Professor of Oriental Languages in the University of Edinburgh, F.R.S.E., &c.

On the afternoon of the 15th February 1837, we sailed from Honáwar* in a light native boat, our baggage following us in another, to Garsapa, which is about sixteen miles due east. The scenery on the banks is most beautiful, and in some places grand. Hills, generally of a considerable size, with their superior rock of laterite, which prevails, more or less, in the low part of the country, approach the edge of the water; and they are covered with vegetation, luxuriant and varied, except where the common bambu (*Bambusa arundinacea*) predominates. It was late when we reached our destination: and it was later still when our horses, which had been led by a circuitous route through the jungle, arrived. Their keepers and the guides maintained that they had been greatly alarmed by the appearance of wild beasts on the way. The sepoys in charge of the travellers' bungalow informed us that they are numerous in the more open parts of the forest, which is not unlikely. Mr Mattley, of the Civil Service, afterwards shewed us the last annual government returns connected with the ferine warfare carried on in the Nagar districts above the gháts. Ninety-two men and 901 cattle had been reported as carried off and destroyed, while rewards had been given for the skins of 100 tigers, 136 chitás, four wolves, and twenty-six bears, which had been killed by the natives. In some parts of the forest wild oxen and *sámbárs* abound. We started one of the latter. It seemed to me, from its great size and dark colour, to be the *Cervus Aristotelis* of Bengal, and not the *Cervus equinus*, which, as mentioned by Colonel Sykes, abounds in the Dakhan. Its antlers, a pair of which we had examined at Honáwar, are very large and heavy.

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* Corrupted by the English and Portuguese into Onnore.

Near Garsapa are the ruins of an ancient town, which our time did not permit us to examine. We started early next morning, and commenced the ascent of the Malikalí Ghat, which, except in a few places, we did not find so difficult as we expected. The forest, which consists of the most lofty trees, on which we observed many troops of monkeys, of the species *entellus*, sporting themselves, is on each side of the path dense and impervious. From an eminence, where, for a few yards, there is an open space, and where travellers generally halt for a little, the view is so magnificent, that a description of it in the plainest prose would have all the effect of the most elevated poetry.

The rocks which we noticed on our way were principally of mica-slate and unmixed hornblende, as at Halíalí, at which we concluded our march for the day, of sixteen miles. On the trees and bushes near the bungalow in which we were sheltered, we observed many birds, principally of the passerine order, which were new to us. Among these, the *Plænicornis princeps* particularly attracted our attention, by its glossy black and brilliant scarlet colours, and its dazzling flittering in the sun. In our neighbourhood there were some gardens of a considerable size, and well watered, for the rearing of betel-nut trees and pepper vines, to which they afford support, and which twist around them. They yield a good produce, and belong to Háiga Brahmans, the most important agricultural class of men in that part of the country. They seem to me to correspond with the Bhátelás of Gujarát, who, though probably not originally of the priestly order, had their present status conferred upon them as an inducement to their acceptance of Hinduism. Those with whom we conversed have the Canarese language vernacular to them, and only a slight knowledge of Hindustané. The only character which they read, is the Devanágarí.

On the morning of the 16th February we started for the Falls. Our road was in a south-east direction, and lay through a beautiful country, containing many cultivated fields, and correctly described by Dr Christie* as "combining the majestic appearance of a tropical forest with the softer character of an

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* Jameson's Journal for September 1828.

English park," and noticed by my intelligent companion as not unlike some parts of the summits of the Nilgiris. We arrived at the bungalow of the Jog, the native name of the Falls, at nine o'clock; and, under the impression that they were not far distant, we repaired to them before we took breakfast, having a walk of three miles. Before describing our impressions connected with them, I shall quote in full the only printed account of them which I have read, and of the existence of which we were not aware till our return to Bombay. It will be observed that, though it is generally correct, we were enabled to extend our observation somewhat beyond that of the able writer.

"Upon approaching the Falls," says Dr Christie, "you emerge from a thick wood, and come suddenly upon the river, gliding gently among confused masses of rock. A few steps more, over huge blocks of granite, bring you to the brink of a fearful chasm, rocky, bare, and black; down into which you look to the depth of a thousand feet! Over its sides rush the different branches of the river, the largest stretching in one huge curling pillar of white foam, without interruption to the bottom. The waters are at the bottom, by the force of their fall, projected far out in straight lines; and, at some distance below the falls, form a thin cloud of white vapour, which rises high above the surrounding forest. The sides of the chasm are formed by slanting strata of rock, the regularity of which forms a striking contrast to the disorder of the tumultuous waters, the broken detached masses of stone, and the soft tint of the crowning woods.

"The effect of all these objects rushing at once upon the sight, is awfully sublime. The spectator is generally forced to retire after the first view of them, in order gradually to familiarize himself with their features; for the feeling which he experiences upon their sudden contemplation, amounts almost to pain. After their first impression has somewhat subsided, and he has become accustomed to their view, he can then leisurely analyze their parts, and become acquainted with their details.

"The chasm is somewhat of an elliptical form. At its narrowest and deepest part is the principal fall; and over its sides smaller branches of the river and little rills are precipitated, and are almost all dissipated in spray before they reach the bottom. The principal branch of the river is much contracted in breadth before it reaches the brink of the precipice, where it probably does not exceed fifty or sixty feet, but it contains a very large body of water.

"The falls can only be seen from above, for the precipices on both sides of the river afford no path to admit of a descent. Some gentlemen have attempted to reach the bottom by having themselves lowered by ropes; but no one, to my knowledge, has hitherto succeeded. A view of the falls from below would, I am convinced, exceed in grandeur every thing of the kind in the world. The spectator can, very easily, and with great safety, look down into the chasm to its very bottom. Some large plates of gneiss project, in an inclined position, from its edge; so that, by laying himself flat upon one of these, he can stretch his head considerably beyond the brink of the precipice.

"No accurate measurement has yet been made of the height of these falls. Some who have seen them declare, that their height reaches at least 1100 feet; others, that it does not reach 1000. I prepared a rope 900 feet long, attached a stone to one end of it, and let it slip over the edge of a rock, which projects several feet beyond the side of the precipice. When 500 feet of rope had been let out, the stone was forcibly drawn towards the principal cascade, which soon involved it among its waters, and snapped the rope. The stone at this time appeared to be about 200 feet from a small ledge of rock, which might be between 200 and 300 feet from the bottom. It is not improbable, therefore, that the height of the fall is not much short of 1000 feet."

I had determined to look upon the Falls, in the first instance, with the greatest philosophical coolness imaginable; and then, after taking a survey of the whole scene, to allow my feelings to run riot as they might please. When, after stepping over some forty or fifty yards of rough and mis-shapen rocks, I had approached the perpendicular cliffs over which the torrents of water are precipitated, as I was essaying to direct my curious vision into the tremendous abyss below, my eye caught my fellow-traveller prostrate upon the very edge of the rocks, with his head protruded beyond them; and I was so suddenly awe-struck, that I instinctively recoiled some yards from my position, and with difficulty would my nerves allow me to resume it. They were again violently agitated by one of our guides,—in a fool-hardiness which he intended should gain our applause, but which we did not commend,—walking upright for several yards along the margin, and tossing up his body, as if threatening to precipitate himself, along with the rushing waters. The dip of the gneiss, however, about five degrees landward, was no sooner observed by me, than I concluded that my centre of gravity would have a safe inclination, and, in the exercise of all my courage, I began to survey the awful chasm. My impressions it is utterly impossible to describe. I gazed in silent wonder, and not, I trust, without adoration of Him who "putteth forth his hand upon the rock, who overturneth the mountains by the roots, who cutteth out rivers among the rocks, and whose eye seeth every precious thing."

Owing to the advanced season of the year, we did not find much water in the bed of the river. At the point of the amphitheatre nearest us, there is a streamlet exactly of a yard in depth and width. It rushes to the termination of the rocks with great velocity, and meeting with not the slightest obstacle in its descent from it, it seems to shoot off like a rocket, and is apparently completely dissipated in vapour long before it has passed through its appointed space. The centre fall has a much larger body of water. In the first instance, it rushes for about three hundred feet over an inclined plane of about forty-five degrees, in a beautiful snow-white foam, and it afterwards descends perpendicularly into the basin below, with a thundering noise. The fall on the southern side, with about the same quantity of water as the preceding, is, about a hundred feet from its commencement, split into two by a projecting rock, but it meets with no other obstruction. It can be viewed in its whole descent with great advantage.

The report of a mass of stone which we threw over, was exactly *nine* seconds in reaching us; but it struck a ledge of rock before arriving at the bottom. We had no means of making for ourselves an accurate estimate of the depth of the fall. It has been lately ascertained by an officer of the Madras Engineers to be 1150 feet, nearly eight times that of Niagara! It does not appear to the eye to be of that extent. The bed of the river is more than a quarter of a mile in the direct line across; and about half a mile, when measured along the margin of the ellipse. In the monsoon the water rushes in every part over this, as we saw from marks on the banks, with a depth of at least thirty feet. The fall is consequently at this season unequalled in the world.

Our desire to descend to the bottom was intense; and though our guides dissuaded us from making the attempt, on account of its great difficulty, they admitted its practicability, of which we had been assured by several friends. After we had taken breakfast, and reclined for a little, our wishes changed themselves into a determination. We crossed the bed of the river, stepping over the rushing floods from rock to rock, not far from the edge of the cliff. On reaching the south bank, we passed through a few small rice fields and topes of jungle, and then commenced our steep descent not many yards distant from the fall, with a sight of which we were occasionally indulged as we proceeded. We had a flight of steps rudely laid in some parts of our journey. In other parts we had to pass over the surface of almost perpendicular rocks, laying hold, for support, of the roots and branches of large creepers. We began to fear that our strength, much of which had been expended in the early part of the day, would really fail us; but, though ready to expire with fatigue, we reached the bottom. Here we rested for a little, and, after dispensing with the greater part of our dress, which was completely soaked with perspiration, we directed our course to the principal basin, scrambling over huge and shapeless masses of rock, which lay scattered in all directions. When we reached our destination, we found the reflected heat almost intolerable, and our thirst excessive. Before our muscles suffered any collapse, however, we entered the water, which proved most refreshing. After bathing, we treated ourselves to a new light dress, and we found ourselves comparatively comfortable, and prepared to enjoy the sublimities which we had descended to witness. Such a scene of wild ruin was never beheld by us. The disrupted rocks piled around us; the dark, frowning, and perpendicular sides of the mountains, which our eye, from our position, could scarcely scale; the boiling, deep, and roaring torrents at our feet; the clouds of spray and vapour in which we were enveloped; the western breeze reverberating in the hollow caverns which obstructed its progress; and the thunder of the falls; all seemed to proclaim that the elements of nature had conspired against us, and filled us with terrific awe! Their voice was, "Hast thou an arm like God, and canst thou thunder with a voice like him? Deck thyself now with majesty and excellency, and array thyself with glory and beauty. Cast abroad the rage of thy wrath, and behold every one that is proud, and abase him. Look on every one that is proud, and bring him low: and tread down the wicked in their place. Hide them in the dust together, and bind their faces in secret. Then will I confess unto thee that thine own right hand can save thee." An undying and beautiful rainbow in the perpetual clouds above us, was, nevertheless, the bow of promise, and might have reminded us of covenanted fidelity and love.

Our ascent was most arduous; and I thought I should never have been able to accomplish it. Our guides procured for us specimens of the granite rocks, which they struck off at different heights, according to our directions. From the bed of the river we ourselves procured several specimens of the superincumbent gneiss, which is finely stratified. From the banks we cut down some branches from cinnamon trees, the first which we had seen. We were grateful when we reached the bungalow in safety; and we were able next day to prosecute our journey to the ancient Banawásí, by way of the celebrated Chandragupti. The mountain on which the fort at this latter place stands is noticed particularly by Dr Francis Buchanan. Banawásí is mentioned by Ptolemy, and has a very ancient temple. Not far from it, we noticed a species of *Buceros*, which, as far as I am aware, is undescribed. It is called by the natives *Danchidi*. We heard the cry of the bird at a great distance, the large hollow excrescence above the beak acting as a sounding-board. We employed two natives to procure for us a specimen, but they were unsuccessful in their attempt to kill one. They gave us, however, two heads which they had in their houses. This bird, they said, lives principally upon fruit, and it is much relished as an article of food.

JOHN WILSON.

BOMBAY, 16*th August* 1837.

Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, chiefly in the Royal Botanic Garden. By Dr GRAHAM, Prof. of Botany.

Dec. 10. 1837.

Begonia parvifolia.

B. *parvifolia;* suffruticosa, glabra; foliis inæqualiter cordatis, lobatis, lobis subacutis, undulatis, sparce serratis, subglaucescentibus; fructibus trialatis.

Begonia parvifolia, *Schott.—Sprengel*, Syst. Veget. c. p. 408.

DESCRIPTION.—Whole *plant* glabrous. *Stem* suffruticose; branches red, transparent. *Petioles* (1–2 inches long) spreading, at length divaricated, channelled above. *Leaves* shorter than the petioles, ovate, unequally cordate at the base, delicate green above, pale and crystalline below, with prominent reddish veins, lobed; lobes subacute, distantly serrated, with a minute bristle in the base of each fissure. *Corymbs* axillary, on peduncles which are longer than the petioles, forked, bearing a male flower in the cleft, and one male and one female upon each division. *Bracteæ* subrotund, greenish-white. *Flowers* white; *male flowers* of two subequal entire and kidney-shaped petals, very rarely with a small one between these; stamens numerous, attached only at the base, filaments slender, connective obovato-elliptical, anthers small, lateral: *female flowers* of five obovate unequal petals; styles short and stout, expanded at the apex, and contorted; stigmata marginal upon the extremity; germen with three unequal, irregular, large, crenate wings.

We received at the Botanic Garden, Edinburgh, this species, with many other scarce plants, from the noble and always liberal establishment at Berlin, in 1836. It flowered profusely in the collection of Dr Neill at Canonmills, and at the Botanic Garden in May, and throughout the summer. The great and long-continued succession of flowers which it produces, and its neat foliage, makes it a desirable species in cultivation.

Begonia sinuata.

B. *sinuata;* caulescens, ramosissima, ubique glaberrima; foliis inæqualiter cordatis, lobatis, acutis, obtuse dentatis, nitidis, subtus pallidis, venis coloratis; floribus masculis dipetalis, staminibus subliberis; fœminiis 5-petalis, inæqualibus; capsulæ alis subæqualibus, acutis; pedunculis bifidis, ramis bifloris.

Description.—Whole *plant* glabrous and shining. *Stem* thick, short, much branched; branches erect, tumid at the joints. *Stipules* attenuated at the apex, dilated at the base, and sometimes half sagittate, marcescent. *Petioles* (2–3 inches long) suberect, channelled above. *Leaves* shorter than the petioles, unequally cordate, lobed, shortly and bluntly toothed, slightly waved, bright green above, paler below, where the veins are deep red, especially on the older leaves. *Cymes* axillary, peduncle about as long as the petiole, slightly compressed, dichotomous, with a male flower in the fork, and one male and one female flower on each division. *Male flowers* dipetalous, white, petals equal and subrotund, stamens united only at the base of the filaments. *Female flowers* with five unequal petals. *Germen* with three subequal acute wings, slightly rose-coloured at the edges near the base when young.

The number of species in cultivation of this beautiful genus has been greatly increased of late years, and a large proportion through the often mentioned Garden at Berlin, from whence this was sent to the Botanic Garden, Edinburgh, and to Dr Neill in 1836, under the name here adopted. In both these establishments it flowered in the end of May and in June. The country from which it was obtained was not stated to us.

Dendrobium aggregatum.

D. *aggregatum;* pseudobulbis cæspitosis, monophyllis, ovatis, sulcatis, stipitatis, cuticulo cinereo vestitis; foliis oblongis, emarginatis, coriaceis, nervosis; racemo laterali cernuo multifloro fere duplo brevioribus; petalis ovatis sepalo latioribus; labello subintegerrimo, latiore quam longo, basi concavo pubescente.

Dendrobium aggregatum, *Roxb.* Fl. Ind. 3. 477.—*Lindl.* in Wall. Cat. 7411.—Bot. Reg. 1695.

Description.—*Pseudo-bulbs* (2 inches long) ovate, crowded, having about eight broad shallow grooves, and prominent subacute ridges, loosely covered with a grey shining wrinkled cuticle, and marked with two transverse lines (joints); the outer stipitate. *Leaf* solitary, oblong, coriaceous, obscurely nerved, emarginate and somewhat unequal at the apex, attenuated at the base, twice as long as the pseudo-bulb. *Raceme* (6 inches long) protruded laterally from the upper joint, cernuous, many-flowered, lax. *Flowers* springing from the axil of a small acute *bractea*, orange-coloured, darkest towards the base of the lip; *sepals* ovate, united at their base, where the two lowest form a little pouch behind the base of the lip; *petals* ovate, broader and longer than the sepals, on short claws; *lip* broader than long, entire in the middle, ciliated towards its base, slightly undulated, concave at the base, and excavated over the point of its insertion into the base of the column, minutely pubescent in front. *Co-*

lumn short, streaked with red in front, having a truncated scale near its base, scarcely hollowed below the anther, at each side of which there is a small tooth, and behind in the centre another, to which the bilobular greenish anther-case is attached; pollen-masses sessile, each grooved along the centre.

The plant was received at the Botanic Garden, Edinburgh, from Wentworth, in 1836, and flowered in the beginning of May 1837. It is handsome, but its blossoms quickly expand, and last a short while, being therefore much less ornamental than the *Oncidiums*, which were in flower with us at the same time, but whose individual blossoms are in beauty for many days.

Galphimia glauca.

G. *glauca;* foliis ovatis, obtusis, glabris, subtus glaucis, basi utrinque unidentatis, petiolis eglandulosis.—*D. C.*

Galphimia glauca, *Cavan.* Icon. et Descriptiones Plantarum, V. 61. t. 489.—*D. C.* Prodr. 1. 582.—*Sprengel,* Syst. Veget. 2. 385.—*Don's* Dictionary of Gardening and Botany, 1. 639.—*Henslow,* in the Botanist, No. 18.

Malpighia glauca, *Pers.* Synops. Pl. 1. 506.

Description.—*Shrub* erect; branches suberect, afterwards pendulous; dark brown, streaked with pale lines from the cracking of the cuticle, *Leaves* opposite, petiolate, ovato-elliptical, glabrous, slightly undulate, uni-dentate on each side near the base, rather opaque green above, glaucous below, middle rib strong, veins conspicuous, both prominent below, and slightly channelled above. *Petioles* spreading, half the length of the leaf, red, channelled above, and, when young, as well as the apices of the branches, covered with adpressed coarse rusty hairs. *Stipules* small, subulate, erect, adpressed to the branch, and connivent above the origin of the petiole. *Raceme* terminal, pedicels springing from the axils of small subulate bracteæ, ascending, as well as the rachis covered with hairs similar to those on the extremities of the branches, jointed in the middle, with two opposite bracteæ below the joint. Flowers falling at the joint (with us without producing fruit) yellow. *Calyx* 5-parted, segments lanceolate, erect, spreading at the apex, herbaceous, without external glands, slightly membranous at the edges. *Petals* 5, ovate, unguiculate, ciliated, spreading; their claws projecting between the calyx segments, middle rib greenish, and prominent behind, before expansion forming vertical angles projecting between the segments of the calyx. *Stamens* ten, of rather unequal length, the shorter opposite to the petals, and half their length; filaments stout, slightly tapering; anthers about half the length of the longest filaments, suberect, projecting with their bases a little way in front of the filaments, slightly notched at both extremities, bursting along the front of the cells, and falling from the filaments at an articulation on the apices of these, which remain upon the flowers, having acquired a red colour after the anthers have dropped. *Stigmata* minute, terminal. *Styles* 3, subulate, diverging. *Germen* 3-lobed, glabrous, round, 3-celled, cells 1-seeded.

This pretty shrub, native of Mexico, we received at the Edinburgh Botanic Garden from Wentworth, and found it to flower very freely in a warm stove in May, June and July 1837.

Gesnera rupestris.

G. *rupestris;* subacaulis, foliis ovatis, cucullatis, rugosis, pubescentibus, inæqualiter crenato-dentatis; corymbis subradicalibus; pedicellis strictis, gracillimis, glanduloso-pilosis.

Gesnera rupestris, *Mart.*

Description.—*Tuber* flattened. *Stem* nearly awanting, supporting two opposite round pubescent petioles, twice as long as itself. *Leaves* (8 inches long, 6½ broad) ovate, peltate, minutely glanduloso-pubescent, strongly nerved, bullate, unequally crenato-dentate, concave and dark above, paler below; *corymbs* subradical, pedicels (2–2½ inches long) erect, very slender, glanduloso-hairy, opposite, decussating, rising from the axil of a linear bractea. *Flowers* deflected outwards. *Calyx* glanduloso-pubescent, 5-cleft, tube fleshy, adherent, teeth subulate, connivent after the corolla falls. *Corolla* (1¼ inch long) tubular, curved, limb of five, subequal, rounded, short, segments, of which the two upper are rather the smallest, and least deeply divided. *Stamens* 4, perfect, slightly exserted; filaments glabrous; anthers dark, rounded, 2-lobular, bursting along the face of the cells, connective, rounded; pollen yellow, granules minute, one, sometimes two rudimental stamens on the upper side of the tube at its base. *Disk* white, projecting on the upper side, emarginate. *Germen* half immersed in the calyx tube, above this conical, slightly pubescent, brown. *Ovules* numerous. *Style* nearly as long as the stamens, slightly pubescent, colourless. *Stigma* bilobular, lobes short.

This singular-looking species was received at the Botanic Garden, Edinburgh, from Berlin in 1834, and has flowered freely in the stove every autumn since.

I have not had the advantage of seeing the description of this species by Martius, nor do I know where it is published.

Lophospermum scandens.

L. *scandens;* foliis cordatis, grosse inæqualiterque dentatis, tenuissime glanduloso-pubescentibus; calycis segmentis oblongis, apice attenuatis; corolla extus nuda, lobis inferioribus erectis.

Lophospermum scandens, *Don*, in Linn. Soc. Trans. 15. 353.—*Henslow*, in the Botanist, No. 17.

DESCRIPTION.—*Plant* herbaceous, scandent by the petioles, with long slender branches, everywhere, excepting on the outside of the corolla, finely glanduloso-pubescent. *Leaves* (1¾ inches broad, 2½ long) opposite below, alternate above, petiolate, heart-shaped, subacute, coarsely and unequally toothed, of lively green above, paler below, 7-nerved, nerves prominent below, channelled above, teeth mucronate. *Petioles* as long as the leaves, grooved above. *Peduncles* solitary, axillary, single-flowered, erect, reaching beyond the middle of the leaf. *Calyx* 5-partite, segments (1 inch long, 5 lines broad) oblong, slightly cordate at the base, attenuated and connivent at the apex, projecting outwards along their edges, entire, slightly undulate, subequal, the lowest rather the shortest. *Corolla* twice as long as the calyx, funnel-shaped, purplish-rose coloured, obscurely dotted on its outside, but without pubescence; tube white, especially on its lower side, flattish above, with two grooves below, dilated and angular where it covers the germen; limb 5-lobed, uppermost lobes largest, subacute, reflected, slightly pubescent within, lowermost blunt, erect, the middle one the smallest, and from its base on each side proceeds to the origin of the longer stamens, an elevated line corresponding to the groove on the outside, and covered with short erect coarse hairs, yellow only towards the limb, and much less dense than in *Lophospermum erubescens*. *Stamens* 4, didynamous, as long as the tube, from the base of which they arise, and to which they are adherent till they pass the germen, above which they are free, and hairy for a little way, higher up glabrous, sprinkled with a few colourless glands only towards the anthers; anthers cream coloured, lobes diverging below, and bursting along the face, pollen granules minute. A minute tuft of hairs between the shorter stamens, and a little below the part at which they become free, is the rudimental condition of a fifth stamen. *Pistil* as long as the shorter stamens; germen small, conical, green, seated upon a yellowish obscurely lobed glabrous disk; style continuous, straight, in contact with the upper side of the tube, glabrous, bent nearly to a right angle immediately below the stigma. *Ovules* very many.

This plant, though only lately introduced, is easily cultivated, either in the open air or in the greenhouse, and has, with the *Lophospermum erubescens*, and the *Rhodochiton volubile*, even, within a few months, given a character to our trellises as new as that given a few years ago to our greenhouses, by the introduction of the Fuchsias, Calceolarias, and the varieties of the genus *Salpiglossis*.

Potentilla glabra.

P. *glabra ;* caule fruticoso erecto ; ramis nutantibus ; foliis pinnatis, bijugis, sursim ternatis vel simplicibus, utrinque glabris nitidis, subtus glaucis (floribus albis.)

Potentilla glabra, *Lodd.* Bot. Cabinet, 914.

DESCRIPTION.—*Shrub* erect, rigid, everywhere glabrous, excepting occasionally on the edges of the leafets, on the young shoots, peduncles, and outside of the calyx, where there are a few long spreading hairs. *Flowering branches* cernuous. *Leaves* very numerous, small, pinnate, bijugous, those towards the flowers ternate, or at the base of the peduncle simple ; leaflets ovato-acute, turned forwards at their apices, reflected at their sides, slightly undulate, veined, shining above, glaucous below, the upper pair slightly decurrent, and often united at their base to the terminal leafet. *Stipules* dry and membranous, brown, 2-ribbed, united in front of the petiole, and about as long as it, bifid at the apex. *Peduncles* (about half an inch long) reflected upwards, collected in small numbers, or solitary near the extremity of the branches. *Petals* subrotund, entire, white, longer than the calyx. *Styles* short and abrupt. *Receptacle* hairy.

This is a very pretty little shrub, producing in abundance its small rose-like white blossoms ; its erect rigid stem, and arched branches, forming a neat round bush, and fitting it for cultivation along with the smaller ornamental species of *Cistus*. The flowers appeared in succession in the open border at the Botanic Garden, Edinburgh, during August and September. The species is native of Siberia, and is stated by Mr Loddiges to have been received by him from Mr Busch of St Petersburgh in 1822. The difficulty of propagating it, noticed by Mr Loddiges, is probably the reason why it is so seldom seen in collections.

Proceedings of the Royal Society.

1837, *February* 6.—Sir THOMAS M. BRISBANE, Bart. President, in the Chair. The following Communications were read :

1. On the Action of Disintegrated Surfaces of Crystals upon Light. By Sir D. Brewster.

In this paper the author commences with a summary of the present state of knowledge as to the internal constitution of crystalline bodies ; and he then proceeds to describe the optical figures produced by the disintegrated surfaces of minerals and artificial minerals, according as the disintegration is effected—1. By the action of solvents while the crystal is forming, or remains in the bowels of the earth ; 2. By the action of solvents on the surface of perfect crystals; and 3. By mechanical abrasion. The crystals of the first denomination he has examined are Brazil topaz, white fluor-spar, hornblende, axinite, boracite, oxidulated iron, dodecahedral garnet, diamond, and amethyst. The perfect crystals which he found best fitted for presenting the phenomena he describes are alum, fluor-spar, and calcareous spar. The facts related in this paper consist of a description of a great variety of optical appearances, of which it is impossible to give an abstract.

2. Observations on the Pulsation of the Heart and Arteries. By Dr Knox.

The author here first describes briefly what has been done by previous physiologists towards determining the differences produced on the pulse by varieties in posture, by digestion, by age, by stature, by the period of the day, and by muscular motion. He then proceeds to relate the particulars of many new observations he has himself made upon these and other points connected with the circumstances which influence the rate of the heart's contractions. The general results are, that in his opinion the pulse is more frequent in the morning than at any other period of the day, abstracting the effect of stimuli; that the heart is more excitable at the same period; that the pulse is depressed by cold; that it is excited by muscular exertion, and more in the weak than in the strong, and also more by exercise carried to fatigue than by moderate exercise; that it is most frequent in the young, and least frequent in the old; and that great varieties are produced by differences in posture, those positions of the body occasioning the greatest excitement which require most muscular effort for their maintenance.

February 20.—Dr ABERCROMBY, V. P., in the Chair. The following communication was read:

I. On an Expression for the effort required to Ascend Planes of Different Inclinations. By Professor Forbes.

The author states that, whilst it has been pretty generally agreed by authors on animal mechanics that the measure of muscular effort is generally expressed by the weight multiplied by the *vertical* height through which it is raised, it is clear that this cannot be universally true, since, in the particular case of horizontal motion, no vertical height can be obtained; and, when the ascent is vertical, it is very unlikely that the same advantage of ascent should be obtained as at lower angles.

The author proposes, by a formula, to represent the length of path described by an average muscular effort at a given angular ascent in a given time, and which, multiplied by the sine of that angle, will give the vertical height gained.

Direct experiments are quoted, to shew the uniformity of vertical ascent within the usual limits, viz. between 12° and 25°. From the known results of tread-mill experiments, it is supposed that the diminution of effective action at a vertical ascent is not very considerable, but may yet amount to about 1100 feet per hour; whilst about 20°, the effective action will be nearly 1500 feet per hour. The horizontal action is estimated at about four miles per hour. The following formula sufficiently represents extensive experiments at various angles of ascent, h = height ascended in English feet in an hour, at an angle α:—

$$h = \left\{ \frac{1900}{\sin.(\alpha + 5^\circ)} - 800 \sin.\alpha \right\} \sin.\alpha.$$

2. Observations on some New Species of British Fishes. By Dr Parnell.

March 6.—Sir Thomas M. Brisbane, Bart., President, in the Chair. The following communications were read:—

1. On the power of the Periosteum to form new bone. By Professor Syme.

The object of the author in this paper is to state certain observations and experiments by which he considers it to be clearly established, that the periosteal membrane, covering the bones, has the power of secreting osseous substance, and of forming new bony matter.

Duhamel, founding on a false analogy, which he supposed to exist between the periosteum and the bone on the one hand, and the bark and wood of trees on the other, was the first to lay down distinctly the theory that bony matter is formed by the investing membrane of the bones. The experiments and observations of *Haller* and others on the progress of ossification in young animals were believed, however, to militate against this idea, and tended rather to establish the doctrine, that new bony matter is formed by the pre-existing or old bone. Between these two theories, the opinions of physiologists and surgeons are still pretty equally divided.

The author was lately induced to suspect, from the circumstances of a case of necrosis, that the theory of Duhamel is the true one.

In a case of inflammation of the tibia consequent on an injury and terminating in death, of nearly the whole of that bone, amputation became necessary at the close of five weeks after the accident. On examining the amputated limb, the periosteum of the tibia was found chequered with patches of osseous substance adhering to its inner surface, and covered with a delicate membrane. This observation led him to subject the question to the test of experiment, which he conceives he has accomplished in the following manner. An incision was made in the fore leg of a dog, and a portion of the radius, about two inches long, was removed along with its investing membrane. In the other fore leg the same operation was performed, with the exception that the periosteum was carefully dissected from the portion of bone which it was intended to remove, and two inches of bone were cut out, its membrane being thus left entire. In six weeks the animal was killed; when it was found that a large vacuity was left in the radius, where the membrane was removed as well as the portion of bone; while in the other, where the membrane had been left, the gap was entirely filled up by firm osseous texture, and at this part of the radius the bone was even thicker than elsewhere. The experiment was several times repeated with the same result. In another dog the periosteum of the radius was carefully dissected off as before, but without either disturbing the position of the membrane or removing the subjacent portion of bone; and a thin metallic plate was passed around the bone between it and the detached periosteum. In six weeks the animal was killed and the limb examined. The inner surface of the membrane presented an extensive deposition of osseous matter between it and the metallic plate; while between the plate and the bone there was merely a membranous formation without any new bony substance whatever.

2. On one Troja. By the Venerable Archdeacon Williams.

The introduction only of this communication was read. The abstract will be given after the paper is concluded.

March 20.—Sir Thomas M. Brisbane, Bart., President, in the Chair. The following communication was read:

On the Action of Voltaic Electricity or Pyroxylic Spirit, and on Solutions in Water, Alcohol, and Ether. By Arthur Connell, Esq.

The author has found a perfect resemblance to exist between the appearances presented by pyroxylic spirit (Bihydrate of Methyline of Dumas and Peligot) under voltaic agency, and those which he had previously observed in regard to alcohol; a coincidence which was to be expected, from the interesting analogy existing between these two liquids in other respects.

The pyroxylic spirit, after three distillations from quicklime, had a specific gravity of 801 at 62° F., and boiled in contact with mercury at 140° F., under a pressure of 29.5 inches. The leading facts observed were the following:—

Minute quantities of acid, alkaline, and saline bodies, when dissolved with rectified pyroxylic spirit, favoured the voltaic action, by increasing the conducting power of the liquid. When it held in solution $\frac{1}{350}$ of pure potash, and was submitted to the agency of thirty-six pairs of four-inch plates in a tube with parallel platinum foil poles, hydrogen was given off from the negative pole, while no gas proceeded from the positive pole. A ten thousandth part of potash had a marked effect in promoting this action.

When the pure spirit was submitted in a similar tube to the agency of seventy-two pairs of four-inch plates, hydrogen was in like manner evolved, although in smaller quantity.

When the same electric current was passed through the spirit and through water, each holding dissolved $\frac{1}{300}$ part of potash, the quantity of hydrogen evolved from both negative poles was the same.

Besides the evolution of elastic fluid, there were found in the liquid acted on small quantities of such substances as often result from the oxidation of analogous liquids, such as resinous matter; and, where the quantity of dissolved potash was considerable, a speedy precipitation of carbonate of potash ensued, the liquid at the same time acquiring a red colour from the formation of resinous matter.

In the whole circumstances it was concluded, as had been done in regard to alcohol, that water, entering as such into the constitution of pyroxylic spirit, is resolved under voltaic agency into its elements, its hydrogen being evolved at the negative pole, and its oxygen employed in giving rise to the secondary effects of oxidation.

As pyroxylic ether (hydrate of methyline of D. and P.) bears the gaseous form, no galvanic experiments were attempted with it; but, as the author had formerly observed no evidence of the presence of water in sulphuric ether under electric agency, he is disposed to adopt the analogous view as in regard to alcohol, and to consider pyroxylic spirit as a hydrate of pyroxylic ether, which latter substance, like sulphuric ether, he regards not as an oxide, but simply as a ternary combination of its constituents.

The results formerly stated to the Society in proof of the secondary origin of chlorine or iodine, when aqueous solutions of the corresponding hydracids and haloid salts are submitted to voltaic agency, have been confirmed with stronger voltaic powers, the only difference being, that the chlorine or iodine, and also the corresponding hydracid, from the secondary decomposition of which, by nascent oxygen, the chlorine or iodine proceeds, are sooner carried over from the negatively electrified solution into the positively electrified water, than when smaller powers are employed. The decomposition of bromide of iodine in an aqueous solution of starch, has also been ascertained, by analogous methods, to be a secondary action.

The appearances presented during the electric decomposition of alcoholic solutions of acids, alkalies, and salts, have been found to be very analogous to those observed in the corresponding aqueous solutions, the principal difference being, that the oxygen of the decomposed water of the alcohol rarely appears at the positive pole, for reasons formerly explained. An ordinary oxyacid salt dissolved in alcohol is slowly resolved, under voltaic action, into its constituent acid and base, with evolution of hydrogen at the negative pole; and, where the base is not of difficult reduction, a portion of it is reduced by the hydrogen. By experiments conducted on principles exactly analogous to those with aqueous solutions, it was found that the appearance of iodine at the positive pole, in an alcoholic solution of iodide of potassium, is a secondary effect due to nascent oxygen.

By instituting a comparison between the electric action on alcoholic and on aqueous solutions of chlorides and iodides, under circumstances in which no secondary actions could take place at the poles, the appearances led to the conclusion, that these salts are not dissolved as such in water, but as muriates and hydriodates. The solution was placed in a tube connected by means of asbestus with two other tubes containing distilled water, the positive pole being introduced into one of the tubes containing water, and the negative into the other. When an alcoholic solution of a chloride or iodide was in the middle tube, no acid was observed to be produced in the alcoholic solution, but only at the point of contact between the solution and the water. On the other hand, when an aqueous solution was employed, a plentiful production of the corresponding hydracid was observed in that solution on its positive side. These appearances are exactly those which ought to be observed, if we suppose chlorides and iodides to be dissolved as such in absolute alcohol, and as muriates and hydriodates in water. In none of these cases was chlorine or iodine produced, except at the positive pole in the positive water, by a secondary action on the corresponding hydracid.

When etherial solutions of various substances, such as potash, chromic acid, and metallic chlorides, are submitted to galvanic action, there is no symptom of decomposition either of the solvent or of the dissolved body. Thus no exception has yet been met with to the general conclusion, "That when solutions of binary combinations of elementary bodies in water, alcohol, or ether, are submitted to voltaic agency, the dissolved substance is not decomposed but only the solvent, if itself an electrolyte." A few cases, however, comprehended within the law still remain to be experimentally investigated.

Sir Charles Bell gave a Verbal Notice regarding the importance of the Circulation in the Medulla Oblongata, and the office of the Vertebral Artery.

April 3.—The Right Honourable Lord Greenock, Vice-President, in the Chair. The following communications were read:

1. Geological Observations on Binny-Crag in West-Lothian, by Charles Maclaren, Esq.

Binny Crag in West Lothian is of compact greenstone, about half a mile in length, and from twenty to one hundred and forty feet in breadth. It extends south and north, and has a small ravine on its west side, above which the highest part of the crag rises about two hundred feet. Three phenomena connected with this crag are worthy of observation. 1. *It has the character of a trap vein which has ruptured the strata in a fluid state.* The dip of the sedimentary rocks on its west side is to west-south-west, on the east side to the east, and in both cases at a pretty high angle. We have here, therefore, evidence of the strata having been ruptured, and a fissure formed; and we have the trap, the rupturing agent, placed on the fissure, and occupying the anticlinal line, or axis of the upheaving movement. According to the received hypothesis respecting the origin of trap, we might expect this appearance to be common, and yet it is rather rare. It forms the exception, while it ought apparently to be the rule. 2. *The trap has mingled with the shale, and affected its structure in a very remarkable manner.* It has enveloped portions of the shale within its mass, and penetrated laterally among the beds to the extent of one hundred feet or more on the east side. On the south flank of the highest part of the crag we have a section exhibiting the relations of the two rocks. The trap has the form of an arch, of perhaps twenty feet radius, with a long straight tabular mass about three feet farther to the east. A mass of shale lies under the arch exhibiting a laminar structure; in the portions contiguous to the trap, its substance is hardened, and the planes of the laminæ, instead of being straight and parallel, are arranged concentrically with the curved surface of the trap which covers them. Portions of shale also lie under and above the thin tabular projecting mass of trap, and these likewise conform themselves to its surface. The inference is, that, independently of the original slaty form of the shale, a secondary laminar structure was induced upon it, by the heat of the fused trap being propagated through it progressively and unequally, each portion as it was dried and hardened, separating from that beyond it. 3. *The crag exhibits a striking example of the effects of denudation.* The north end of the ridge, which is low, and on a level with the surface of the land westward, has merely had its covering of shale stripped off, and exhibits a few trifling indentations; but the south end, which rises in bold relief above the adjacent land, has been cut into seven distinct hillocks, separated by transverse ravines, which are steep in the sides, and from ten to sixty feet in depth. The appearance of these ravines shews, that they must have been cut by powerful currents flowing from the west.

2. Notice of Experimental Researches into the Laws of certain Hydrodynamical Phenomena that have not hitherto been reduced into Conformity with known Laws. By John Scott Russell, Esq. Lecturer on Natural Philosophy.

April 17.—Sir HENRY JARDINE in the Chair. The following communications were read:

1. On the Constitution of the Earth's Atmosphere in Remote Geological Epochs. By Professor Johnston, Durham.

This is the first paper of a promised series on the subject here announced. In the present paper the author endeavours to shew, that in remote epochs the atmosphere was more extensive and heavier than it is now, that it contained a greater absolute quantity of oxygen, and that this quantity has gradually diminished up to our own era, and is probably still undergoing a sensible diminution. His views are founded partly on speculative considerations, of which it is not possible to give a sufficiently circumstantial abridgment, and partly on the evidence of various operations going on at or near the surface of the earth, the tendency of which must, on the whole, apparently be to diminish the quantity and proportion of the oxygen in the atmosphere. In addition to the ordinary and well-known causes of deterioration, the author points out a new source of diminution to which his attention has been lately drawn. From experiments made upon the aëriform fluid discharged from the earth during an inundation, and obviously expelled by the water penetrating to a considerable depth, and displacing the gases contained in the soil, he found that this gaseous matter consists of 2.5 per cent. carbonic acid, 12.764 oxygen, and 84.736 nitrogen. Taking this observation in conjunction with many others previously made on the composition of the gaseous discharges in volcanic districts, and the gaseous contents of mineral waters, he infers that a process of oxidation is constantly going on at various depths below the earth's surface, the oxygen for which is, in all probability, derived by absorption of the atmospheric gases at the surface.

2. An Account of a New Species of British Bream, and a Species of Skate new to Science; with a List of, and Observations on, the Fishes of the Frith of Forth and Neighbourhood. By Richard Parnell, M.D., F.R.S.E.

3. Notice on the Composition of the Right Prismatic Baryto-Calcite, the Bicalcareo-carbonate of Baryta of Dr Thomson. By Professor Johnston, Durham.

4. An Explanation of the Aristotelian Expression Μετα τα Φυσικα, with some inferences from the Explanation. By the Venerable Archdeacon Williams.

Proceedings of the Wernerian Natural History Society.

(*Continued from* vol. xxi. p. 163.)

SESSION 1836-7.

THE Thirtieth Session of this Society commenced on the 3d December 1836, Professor JAMESON, President, in the Chair.

A Report from the Joint Prize Committees, dated 26th November 1836, was read, approved of, and sanctioned. It was of the following tenor :—

The Committees were appointed on 9th January last, to examine the communications presented to the Society in consequence of their offer of honorary Premiums for the best Geological Report of the Lothians, and best Account of the Fishes of the district of the Forth; and having considered the recommendation of the Council, that the Premiums should be awarded if the communications were found meritorious, even although there should be no proper competition,—unanimously agree to report to the Society, 1. That the honorary Premium of Twenty Sovereigns, or a piece of Plate of that value, should be awarded to Robert James Hay Cunningham, Esq., for his Account of the Lothians, with Maps, Sections, and Specimens; it being understood that the Author will, at his conveniency, furnish the Society with a particular account of the Faults, Organic Remains, and Mines of the district; and, 2. That the honorary Premium of Ten Sovereigns, or a piece of Plate of that value, should be awarded to Dr Richard Parnell,—upon a selection from the dried Specimens submitted to the Society on 30th April last, and containing such species as are mentioned by the Author as new to the district, or which may be considered in any way doubtful, being presented for the Museum of the Society.

The following gentlemen were elected Office-bearers of the Society for 1837:—

President.

ROBERT JAMESON, Esq. F.R.S.L. and E., Regius Professor of Natural History in the University of Edinburgh.

Vice-Presidents.

T. S. TRAILL, M.D., F.R.S.E.
ROBERT STEVENSON, Esq. F.R.S.E.
JOHN COLDSTREAM, M.D.
DAVID FALCONAR, Esq.

Secretary.—PATRICK NEILL, LL.D, F.R.S.E.
Assistant Secretary.—T. J. TORRIE, Esq. F.R.S.E.

Treasurer.—A. G. ELLIS, Esq. *Librarian.*—JAMES WILSON, Esq. F.R.S.E.
Painter.—P. SYME, Esq. *Assistant Painter.*—W. H. TOWNSEND, Esq.

Council.

JAMES YOUNG, Esq.
WALTER ADAM, M.D, F.R.C.P.
WILLIAM COPLAND, Esq. F.R.S.E.
WILLIAM MACDONALD, M.D., F.R.S.E.
R. K. GREVILLE, LL.D., F.R.S.E.
MARTIN BARRY, M.D., F.R.S.E.
JOHN SLIGO, Esq. F.R.S.E.
R. J. H. CUNNINGHAM, Esq.

At the same meeting, the following gentlemen were elected members of the Society:—

Dr Robert Paterson, Leith.
Captain Henry Drummond, H.E.I.C. Service.
William Stanger, Esq., Wisbeach.

At this meeting, Dr Paterson gave an account of the fossil fishes found in the strata belonging to the coal formation at the beach between Newhaven and Granton; illustrating his communication by specimens. And Dr Traill read a notice regarding the poisonous bug of Persia; and exhibited specimens sent home by Dr Bell from Miana.

1836, *Dec.* 17.—Professor JAMESON, P., in the Chair. Mr Torrie, assistant-secretary, read Dr Paterson's account of the fossil plants found in the coal-formation at Wardie; while Dr Paterson exhibited to the members the illustrative specimens.

Dr Barry then read a paper on the unity of structure in the animal kingdom, illustrating the same by large diagrams. (This paper has since appeared in this Journal, vol. xxii. p. 116.)

1837, *Jan.* 21.—DAVID FALCONAR, Esq. V.P., in the Chair. The Secretary read a note from Professor Jameson relative to a splendid specimen of sun-fish, weighing 217 lb., and apparently a new species, lately captured in Leith Roads, which was exhibited to the meeting. —Mr William Jameson then read a notice, regarding the occurrence of the Sacred Ibis, *Ibis religiosa*, in New South Wales, and exhibited specimens from that region.—Dr Robert Hamilton then read an account of the fur-seal of commerce, found at the New Shetland Islands by the late Captain Waddel, shewing that it is identical with the *Otaria falklandica*, first described by Mr Pennant.

At this meeting, James Smith, Esq. of Jordanhill, was elected a member of the Society.

4*th February*.—ROBERT STEVENSON, Esq. V. P. in the Chair. Mr Torrie, assistant-secretary, read a letter from Captain Alexander, the traveller, dated Doorne River, 30°.40′ south latitude, Africa, affording some interesting information. Likewise a letter from David Macadam, Esq. dated H. M. Ship Portland, Athens, May 1. 1836, giving an account of the progress, at that date, of the restoration of that ancient city, accompanied with a copy of a book published at Athens, containing many inscriptions not hitherto published, and likely to be soon obliterated; and also with a meteorological table of the weather on the coast of Helas. Dr Neill read an account of a curious kind of monstrosity observed in the common garden wall-flower; communicated by Dr Paterson, with specimens of the anomaly. Mr Torrie then read the Rev. Mr Robertson's account of the geology of Inverkeithing. Mr Torrie likewise read the Reverend Edward Stanley's account of the analysis of the mineral waters of Rippoldsau. It was then proposed to the meeting to address a memorial to the Commissioners for Northern Lights, praying them to establish daily tide-level observations at the different light-house stations where suitable situations might present themselves for that purpose. This was unanimously agreed to, and Professor Jameson, Mr Smith of Jordanhill, and the Secretaries, were appointed a committee, with powers to prepare and present such a memorial.*

18*th February*.—Dr T. STEWART TRAILL, V.P. in the Chair.

Mr Torrie, assistant-secretary, read Mr William Jameson's critical examination of Mr Swainson's account of the distribution of British birds, his observations being founded on the examination of numerous specimens in the University Museum. Professor Jameson then read a notice regarding the occurrence of Arragonite near Ely and Crail, and more lately at Lochgelly, in secondary trap rocks. Professor Jameson likewise gave an account of a series of rocks collected in the Caucasian range by Major-General George Wright. There were exhibited to the meeting a very fine specimen of the red orang-outang of Borneo, the great sloth from South America, a new species of eagle from Northern India, and the great Californian vulture.

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* In consequence of the above resolution, the following memorial was addressed by the committee to Charles Cunninghame, Esq., Secretary to the Commissioners of the Northern Lights:—

SIR,

We have been appointed a committee by the Wernerian Natural History Society of Edinburgh, to represent to the Commissioners of the Northern Lights the importance of an accurate determination of the relative levels of sea and land on the British coast, and to request respectfully, but earnestly, that the Commissioners would order daily observations of the levels of high and low water to be made and registered at the light-houses best adapted to such experiments. Trusting that you will lay this expression of our desire for the promotion of so important an object before the Commissioners, &c.

To this memorial the following answer was received:—

NORTHERN LIGHTS OFFICE, EDINBURGH,
27*th April* 1837.

GENTLEMEN,

I have to acknowledge the receipt of your letter of the 19th, containing a request, on the part of the Wernerian Natural History Society, that the Commissioners of the Northern Light-Houses would order daily observations of the high and low water to be made and registered at the light-houses best adapted to such experiments. I am directed by the Commissioners to acquaint you, that they will have great pleasure in complying with your request, and that they have made a remit to Mr Stevenson, their engineer, to make the requisite arrangements, and to correspond with you on the subject. I am, &c.

(Signed) C. CUNNINGHAME, *Sec.*

PROFESSOR JAMESON and Committee
of Wernerian Natural History
Society, College.

4th March.—DAVID FALCONER, Esq., V.P. in the Chair. Mr Torrie, assistant-secretary, read Mr Hamilton Stirling's observations on the Punnah diamond mines. Professor Jameson communicated a notice regarding the granite at Kingston Harbour, Dublin, and the trap rocks of the islet of Pladda, off Cantyre, with specimens transmitted by Mr Stevenson, civil engineer. Mr Torrie then read a notice regarding recent marine shells found in a bed of clay thirty feet above the present level of the Frith of Forth, by Mr James Nicol, Polmont. Mr Smith of Jordanhill communicated a letter from the Rev. David Landsborough of Stevenston, describing a deposit of similar recent shells mixed with sea-weed at a similar elevation above the present level of the Frith of Clyde.

After a lengthened conversation on the subject of the entire removal of the Government Trigonometrical Survey from Scotland to Ireland, the meeting agreed to remit to the Council of the Society to prepare a Memorial to Government, requesting that the triangulation of Scotland should speedily be completed and published. And for this special business, the meeting directed that Mr Smith of Jordanhill and Mr James Stewart Menteath, jun. of Closeburn, be summoned to the meeting of Council.

Professor Jameson then laid on the table Lord Gray's Kinfauns Meteorological Table for 1836, and also that of the Rev. Mr Macritchie of Cluny: and exhibited a skeleton of a common cock, shewing a curious abnormal formation in that bird.

25th March.—DAVID FALCONAR, Esq., V. P. in the Chair. Mr Torrie, assistant-secretary, read a communication from the Rev. Samuel Traill, on the mode of ascertaining the rate of the increase of the internal temperature of the earth; likewise an account of experiments made by Mr Peter Grant on the new substance named Donium, found in the Davidsonite of the Aberdeen quarries, communicated by Professor Fleming of King's College, Aberdeen. Dr Martin Barry then read further observations on the unity of structure in the animal kingdom, and on congenital anomalies, including hermaphrodites; with remarks on embryology, as facilitating animal nomenclature, classification, and the study of comparative anatomy; illustrating the whole by diagrams. (*Since published in this Journal,* vol. xxii. p. 345.)

8th April.—Dr T. S. TRAILL, V. P. in the Chair. It was intimated that the Council had passed a resolution, directing the Secretary to write to the Secretaries of the Royal Society of Edinburgh, and the Highland and Agricultural Society, suggesting the importance of co-operation on the part of the principal scientific associations, and especially of these societies, in an application to Government for the resumption of the trigonometrical survey of Scotland. Mr Torrie, assistant-secretary, read the first part of Captain Mackenzie's account of his overland journey from India. Mr Smith of Jordanhill read an account of some extraordinary optical phenomena depending on atmospheric refraction, observed in the counties of Ayr and Stirling. Mr Macgillivray then read a paper on the geological relations, and animal and vegetable productions, of the Cromarty Frith, with observations relative to the estuaries and sea-lochs of Scotland.

22d April.—The following Memorial, prepared by the Council and Messrs Smith of Jordanhill and J. Stewart Menteath, jun. of Closeburn, was read and approved of.

Unto the Right Honourable the Lords Commissioners of His Majesty's Treasury, the Humble Memorial of the President and Members of the Wernerian Natural History Society of Edinburgh;

Sheweth,

THAT while your Memorialists view, with the utmost satisfaction, the progress which has been made in the noble Ordnance Surveys of England and Ireland, and are fully alive to the immense advantages which those parts of the Empire are already deriving from the admirable Trigonometrical operations by which their physical geography has been defined, your Memorialists beg leave humbly, and most respectfully, to urge upon your Lordships' attention the very defective state of the best existing Maps and Charts of Scotland, and to suggest to your Lordships the propriety of directing the resumption of the Triangulation, and completion of the Trigonometrical Survey, of SCOTLAND, which has been so long and unaccountably suspended, after it had been auspiciously commenced.

The errors in Arrowsmith's Map of Scotland, which has the reputation of being the best we possess, are so numerous and important as to render the construction of a Geological Map of the country, on which dependence can be placed, an impracticable undertaking; while its erroneous positions of our Coasts and Islands present the most formidable obstacles to navigation. The form and position of headlands, and even of considerable islands, in this map, and in our best charts, are erroneously given; and sometimes dangerous rocks and whole islands are totally omitted. For example, your Memorialists beg leave to call your Lordships' attention to the following facts. The distant rocks of the *Stack* and the *Skerry*, off the northern coast of Sutherlandshire, as well as the Island of *St Kilda*, are totally omitted in Arrowsmith's Map, while the important Islands of Barra and Rona are misplaced, both in latitude and longitude. In some charts the large Island of Arran is laid down as 6 *miles* from Bute; in others as 9 *miles*, and in a third as 12 *miles* distant from that island. Pladda Island Light, in charts, is placed as 16′ N. of Ailsa Craig; whereas its true distance is only 10′ 20″. These last are serious errors at the entrance of so important a river as the Clyde.

Some years ago Dr Macculloch was employed, at the public expense, to make a Geological Survey of Scotland, a circumstance utterly unknown to any public body in Scotland, until the Parliamentary papers shewed that he had drawn from the Treasury upwards of L. 7000 for that service. But the only fruit of this expenditure is the publication of his posthumous Geological Map, on which unfortunately little reliance can be placed for local details, as a trigonometrical survey should have preceded any attempt to represent the position of the rocks and mineral productions by a coloured map. Of this truth Dr Macculloch appears to have become sensible, when he advanced the imperfection of our best maps as an excuse for his delay in preparing the materials he had collected.

Your Memorialists do not consider it necessary to enter into any detailed observations on this occasion on the numerous and important advantages which must result to navigation, commerce, and agriculture, or the scientific interest which would arise from the completion of the Trigonometrical Survey of Scotland; as your Memorialists have no doubt that these are obvious to your Lordships; and they have the fullest confidence in the desire of his Majesty's Government to extend the benefits of accurate geographical knowledge to all parts of his Majesty's dominions.

Signed in name of the Society,

R. Jameson, *President.*

University, Edinburgh, 26th April 1837.

The following Minute of Council was also read:—"The meeting directed that 100 copies of the Memorial be printed; that a fair copy be written out for the Treasury, to be signed by the President in name of the Society, and transmitted through a Member of Parliament: and that afterwards, a printed copy be transmitted to each of the Lords Commissioners, to the Speaker of the House of Commons, and to each of the Scotch Members of Parliament," &c Mr Torrie, Assistant Secretary, read a continuation of Captain Mackenzie's account of his overland journey from India, particularly describing the present statistics of Mocha. Also an account of a visit, last summer, to the Jardin, near Chamouni, with a list of alpine plants. Mr Kemp then exhibited some interesting experiments with potassium, producing readily the metallic bases of various earths, and shewing how the sudden inflammation of the potassium, on coming in contact with water, might be rendered useful in affording an instantaneous though transient light in a dark night at sea.

The Society adjourned till November next.

Proceedings of the Society for the Encouragement of the Useful Arts for Scotland.

11*th January* 1837.—Robert Bald, Esq. V. P. in the Chair.—The following communications were laid before the Society:—

1. Part Second of the Annual Report on the State of the Useful Arts, ordered by the Society. By Edward Sang, Esq. F. R. S. E., late V. P. Soc. Arts, teacher of Mathematics, and lecturer on Natural Philosophy, Edinburgh.

> In this paper Mr Sang took into consideration the relative degrees of skill needed in hand and slide turning; and shewed that the introduction of slide-rests, had brought to light difficulties in obtaining minute accuracy, which before were hardly felt.

2. Model and Description of a Scraper, to be drawn by horses, for scraping the Sludge off Turnpike Roads; and also Model of a Snow-Plough, to be drawn by men, for clearing new-fallen snow from side-pavements of streets, or from the walks in pleasure grounds. Invented by Mr Thomas White, wright, Straton.

3. Supplement to a former paper on the Use of Steam in the economizing of Fuel. By Dr Andrew Fyfe, F.R.S.E., M. S. A., lecturer on Chemistry, Edinburgh.

4. Drawing and Description of a Pendulum Escapement. By Mr Andrew Symington, watchmaker, Kettle, Fife.

5. Notice and Specimen of Flowered Muslin, cut off the first yard of cloth ever flowered by machinery. The machinery invented by Mr John Morton, Marchburn Cottage, Dunoon.

6. *Donation.*—Report to a Committee of the Commissioners of the Northern Lighthouses, appointed to take into consideration the subject of illuminating Lighthouses by means of Lenses;—On the New Dioptric Light of the Isle of May. By Alan Stevenson, Esq. M. A., civil-engineer. Presented by the author.

7. The Report of the Committee on Mr Kay's Turnip Cutter, was read, and approved of.

The following candidates were admitted ordinary members, viz:—

George Irving, Esq. W. S. 106 Prince's Street; Mr George Aikman, engraver, 29 North Bridge; Mr John Burke junior, upholsterer and cabinet-maker, 8 St Andrew's Square.

25*th January* 1837.—Robert Bald, Esq. V. P. in the Chair.—In consequence of the absence of the Secretary, and of some of the gentlemen who were to have read communications this evening, by reason of the prevailing epidemic, the

Society did no business, but resolved to adjourn till the 15th proximo, being the next ordinary meeting.

15th February 1837.—Sir John Graham Dalyell, V. P. in the Chair.

1. Notice of Observations made on the rapidity of Motion, and on the duration of the stoppages on the Liverpool and Manchester Railway. By Edward Sang, Esq. F.R.S.E., late V.P.S.A., lecturer on Natural Philosophy, &c. Edin.—In the course of this paper, Mr Sang expressed his opinion that the present rapid rates of motion on railways may yet be far surpassed.

2. Description of an Improvement in the common Vice, whereby the action of the Screw is made perfect. By Robert Wilson, Engineer, Paul's Work, Edinburgh.—The Vice was exhibited.

3. Model and Description of a Machine intended to be used as a Ship's Windlass; planned and constructed by Mr Andrew Monro, Limekilns, Fifeshire. Communicated by Mr James Dowie, 57 Frederick Street, M.S.A.—A Working Model was exhibited.—The Society seemed highly delighted with this simple contrivance, which enables the seamen to heave up continuously, without stopping to shift the cable.

4. The Committee on Mr J. Kirkwood's Tenoring and Sash-making Machine, reported.

5. *Donation.*—Specimen of Type Printing, in which the New Testament, Psalms, and Paraphrases, are printing in relief.—for the Use of the Blind. By John Alston, Esq. Glasgow, printed by George Brookman. From Mr Alston.

The following candidates were admitted ordinary members, viz :—

1. John Brash, Esq. Secretary, North British Insurance Company, 1 Hanover Street; 2. Thomas Astley, Esq. Manufacturing Chemist, Bonnington. Joseph Whitworth, Esq., Engineer, Manchester, was elected an honorary member.

1*st March* 1837.—Robert Bald, Esq. V.P. in the Chair.—The following communications were laid before the Society :

1. Description and explanation of Avery's high pressure Rotatory Engine; and also a new double pressure Engine. By Mr James Gall, junior, printer, Edinburgh, M.S.A.

2. Mr Ruthven exhibited a working model of Avery's American high pressure steam-engine.

An interesting debate ensued upon the subject of these Rotatory steam-engines; which was adjourned till next meeting.

3. Notice of the architectural arrangements proposed by Mr Alexander Murray, of the Caledonian Pottery, Glasgow, for promoting general education, combined with religious instruction. By Thomas Murray, Esq. L.L.D., Edinburgh. Communicated by Dr D. B. Reid, Couns. S.A.—A large model was exhibited.

4. *Donation.*—Report of the Directors of the Liverpool Mechanics' Institution, in March 1836. From the secretary to that institution, per E. Sang, Esq.

15*th March* 1837.—Robert Bald, Esq. V.P. in the Chair.—The following communications were laid before the Society:

1. Model and an Account of a New Voltaic Battery, being a modification of the construction recommended by Mr Faraday. By Mr James Young, Chemical assistant in the Andersonian University, Glasgow.—Mr Young exhibited the Battery in action, and performed a number of interesting experiments, shewing the power of this Battery.

2. Part first of a dissertation on the Fallacies of the Rotatory Steam-engine. By John Scott Russell, Esq. M.A., F.R.S.E., Couns. S.A.

3. Mr Sang exhibited the Model Steam Boat, constructed for the late Professor Leslie, on the principal of Barker's Mill.

4. Notice of the Theoretical Power of Barker's Mill. By Edward Sang, Esq. F.R.S.E., Couns. S.A.

5. Mr Ruthven again exhibited the Working Model of Avery's American high pressure Steam-engine.

6. The large model of the Architectural Arrangements proposed by Mr Alexander Murray, of the Caledonian Pottery, Glasgow, for promoting general education, combined with religious instruction. Communicated by Dr D. B. Reid, Couns. S.A. was again exhibited.

The following candidate was admitted an ordinary member, viz:

John Bell, junior, Esq. W.S. 13 Great King Street.

29*th March* 1837.—Alexander Adie, Esq. F.R.S.E. in the Chair.—The following communications were laid before the Society:

1. A vertical Orrery on an extremely simple principle, calculated for being used in Seminaries; having only one Wheel and Pinion, and yet exhibiting the motion of the Earth round the Sun, and of the Moon round the Earth: the Parallelism of the Earth's Axis, and the Phases of the Moon. By the Rev. George Tough of Aytoun, Assoc. Soc. Arts.—The Orrery was exhibited and described by Mr Tough.

2. Part second of a dissertation on the Fallacies of the Rotatory Steam-engine. By John Scott Russell, Esq. M.A., F.R.S.E., Couns. S.A.— In this second part Mr Russell considered the practical difficulties in constructing rotatory steam-engines, as also the want of durability occasioned by the unequal motion of the parts.

3. Notice of the Theoretical power of Barker's Mill. By Edward Sang, Esq. F.R.S.E., Couns. S.A.

4. Mr Gall exhibited a working model illustrative of the principle of Avery's American high pressure steam-engine.

5. Model, Drawing and Description of a Rotating Steam-engine. By Mr James Clark, Craig Court, Argyle Street, Glasgow.

6. The adjourned debate on Mr Gall's description and explanation of Avery's high pressure rotatory engine, and of Mr Gall's double pressure engine was resumed.

12*th April* 1837.—Sir John Graham Dalyell, V.P. in the Chair.—The following communications were laid before the Society:

1. Essay on the construction, power, and advantages of Hiero's steam-engine. By Edward Sang, Esq. F.R.S.E., Couns. S.A.—Mr Sang endeavoured to shew, in opposition to an opinion he formerly expressed, that this engine working in vacuum, is capable of giving out the whole power of the steam.

2. An answer to Mr Russell's "Dissertation on the fallacies of the rotatory steam-engine," and to the theoretical objections to Avery's and the new vacuum engine; with further suggestions as to their improvement. By James Gall junior, Couns. R.P.S., M.S.A.

3. Description and Drawing of a new method of preventing explosions in steam-boilers. By Mr John Macfarlane, Fountainbridge.—A model was shewn in illustration.

4. *Donation.*—Mr Gall, junior, presented three copies of a recent work by him on the "Education of the Blind," with specimens of printing for the Blind in the fretted character; also of pictures for the Blind.

5. Specimens of Flowers modelled in wax. By Mrs Brydie, teacher in the female department of the Academy at Dollar; were exhibited and much admired for their fidelity in form and beauty of colour.

James Smith, Esq. of Jordanhill, F.R.SS.L. & E., F.G.S. &c. was admitted an ordinary member.

The annual abstract of the funds under the management of the Society—being for the year 1835–36—was laid on the table.

26*th April* 1837.—Robert Bald, Esq. V.P. in the Chair.—The following communications were laid before the Society:

1. Model of a Waggon and Railroad Switches, shewing a method of opening and shutting the switches by means of the waggon itself, in crossing from one line of railways to another, at the pleasure of the waggoner. By Mr David Kemp, Smith, Fox's Lane, Leith.

2. Descriptions and Drawings of a new method of communicating motion to a shaft lying at an angle. Invented by Mr James Milne, brass-founder, Edinburgh, Couns. S.A.—A brass working model presented to the Society by Mr Milne was exhibited.

3. Description and Drawing of a plan by which Fuel may be economised By Mr Andrew Symington, Watchmaker, Kettle, Fife.

4. On the Mucilage of the Fuci, with remarks on its application to economical ends. By Mr Samuel Brown junior, Haddington. Communicated by Mr Alexander Bryson, Edin. M.S.A.

6. Essay on the selecting, working, and tempering of Steel. By Mr Wil-

liam Nicol, 4 East Adam Street, Edinburgh.—Illustrative Specimens were exhibited.

6. The report of Committee on Mr Symington's Escapement, read and approved of.

7. The report of Committee on Mr Munro's Windlass, read and approved of.

8. *Donation.*—Nos. 11–14 inclusive of the Journal of the Bahama Society for the diffusion of knowledge. From the Bahama Society.

9. *Donation.*—Report by a Committee of the Royal Society of Edinburgh regarding the New Dioptric Light of the Isle of May. Dated 28th October 1836. Presented by John Robison, Esq. Sec. R.S.E., M.S.A.

10*th May* 1837.—Sir John Graham Dalyell, V.P. in the Chair. The following communications were laid before the Society:

1. Specimen of printing on both sides of the paper, for the use of the Blind;—with a few remarks on its advantages, and probable influence on the price of books for the blind. By Mr James Gall junior, printer, Edinburgh, M.S.A.

2. Verbal notice relative to an oversight in Professor Babbage's table of Logarithms, which may lead to erroneous conclusions. By William Galbraith, Esq. teacher of Mathematics, Edinburgh, M.S.A.

3. Description and Drawing of a new instrument called the Harmonometer or measure of Harmony, exhibiting the nature of the major and minor keys of musical composition; the notes belonging to each and all of them, and the relation between every major and its corresponding minor. Invented by Mr John Gray of Henderson Row, Edinburgh.—The description is printed, but was unpublished at the date of lodging. The Harmonometer was exhibited.

4. Notice of an improved Kitchen Lamp. By Mr Edward Roxbey, basket-maker, Glasgow. With a drawing.

5. Model in Plaster of Paris and Drawing of a Dome Arch; with a new method of applying the rules in working the beds of the arch stones. By Mr John Weir, venetian blind-maker, Glasgow.

6. Model, Description, and Drawing of an Inside Pendulum Escapement. By Mr Duncan Macgregor, Smith, Comrie. Communicated by the Rev. D. T. K. Drummond, M.S.A.

7. Model in Brass, and Description and Drawing of an Equable Flow-cock for bottling liquors without disturbing the sediment in the cask, and without loss of time. By Mr John Lothian, 21 Atholl Crescent, Edinburgh.

8. *Donation.*—A Map of the Levels of Edinburgh and its Environs. By Mr William Moffat, Knightsbridge, London. Presented by Mr Moffat.—A specimen frame for ditto, by Mr Alexander Crichton, printseller, West Register Street, M.S. A. was exhibited.

9. *Donation.*—Printed Description of a patent Screw Mooring Machine for mooring vessels. By Mr Dunn, Cur. Soc. Arts.

10. *Donation.*—Printed Essay on the economical manufacture of Tiles and Tile Soles for draining. By Mr John Coulter, builder and tile manufacturer, Glasgow. Presented by Mr Coulter.

11. A Tuning Fork with a scale engraved on it, so that any note may be struck at pleasure. By Mr J. B. Mould, engraver, Edinburgh, M.S.A.

The following candidates were admitted ordinary members, viz:

Mr Alexander Cowan, paper-maker, Moray House, Edinburgh, and H. S Alves, Esq. 9 Royal Terrace.

24th May 1837. Robert Bald, Esq. V.P. in the Chair.—The following communications were laid before the Society:

1. Thirty-two specimens of Transfer and Chalk Lithographic and Zincographic Printing. By Messrs Allan and Ferguson, Lithographers, Glasgow.

2. Twenty-two specimens of Transfer and Chalk Lithographic printing. By Messrs Maclure and Macdonald, Lithographers, Glasgow.

3. Model and Description of a new Paddle Wheel. By Lieutenant A. M. Skene, R.N. Communicated by John Scott Russell, Esq. M.A., F.R.S.E., Couns. S.A.

4. Additional Remarks on the Use of Steam in Economizing Fuel. By Andrew Fyfe, Esq. M.D. F.R.S.E., M.S.A.

5. On some improvements in the construction of Galvanic Troughs. By Andrew Fyfe, Esq. M.D., F.R.S.E., M.S.A.—The improved Troughs were exhibited in action.

6. On the Advantages of increasing the Adhesion of the wheels of Locomotive Carriages to the rails; and on a method of increasing that adhesion; and also of preventing the wheels from starting off the rails. By Mungo Ponton, Esq. W.S., F.R.S.E., Couns. S.A.—With drawings and Models.

> Mr Ponton proposed to create a powerful attraction between the iron rail and the tire of the wheel, by means of electro-magnetic apparatus, thereby causing an increased adhesion without altering the weight of the carriage.

7. *Donation.*—Four specimens of the new Swedish Bank Notes, stereotyped and printed on paper of a peculiar kind, and one specimen of the old notes, shewing the progressive improvement of the useful Arts in the North of Europe. From Robert Bald, Esq. F.R.S.E., V.P.S.A

8. Mr Bald exhibited a specimen of Swedish Cutlery, and specimens of a new coinage of Silver in Sweden, and made some observations on the Swedish Mint, from actual observation.

9. *Donation.*—On the Temperature and Geological Relations of certain Hot Springs, particularly those of the Pyrennees; and on the verification of Thermometers. By James D. Forbes, Esq. F.R.SS.L. & E., &c. Printed in the Phil. Trans. 1836. From the author.

10. *Donation.*—Note relative to the Supposed Origin of the Deficient rays of the Solar Spectrum; being an account of an Experiment made at Edinburgh during the Annular Eclipse of 15th May 1836. By James D. Forbes, Esq., F.R.SS.L. & E. Printed in the Phil. Trans. 1836. From the Author.

11. *Donation.*—Accounts of some Experiments made in different parts of Europe on Terrestrial Magnetic Intensity, particularly with reference to

the effect of Height. By James D. Forbes, Esq., F.R.SS.L. & E. &c. Printed in the Trans. of the Royal Soc. of Edin. 1837. From the Author.

12. *Donation.*—Specimens of Ink and of Chalk Lithography. By Messrs Leith and Smith, Lithographers, 30 Hanover Street, Edinburgh. From Messrs Leith and Smith.

The following candidate was admitted an ordinary member, viz:

Mr William Cooper, patent glass manufacturer to the King, Picardy Place, Edinburgh.

21*st June* 1837.—John Shank More, Esq. Counsellor, in the Chair. The following communications were laid before the Society:

1. Part third of the annual report on the state of the Useful Arts—ordered by the Society:—being on the progress of exactitude in the manufacture of machines. By Edward Sang, Esq. F.R.S.E., Couns. S.A.—teacher of mathematics, and lecturer on Natural Philosophy, Edinburgh.

2. Three specimens of Zincography drawing and printing. By Messrs Allan and Ferguson, Zincographers, Glasgow.

3. The standing committee on the best alphabet and method of printing for the use of the Blind, gave in their report, which was read and approved of, and ordered to be printed in the Society's transactions.

4. Report of the committee on Mr Gall's printing on both sides of the paper for the use of the Blind, was read and approved of, and ordered to be printed along with the relative communication on that subject, and on the fretted type, as an appendix to the principal report on printing for the Blind.

5. *Donation.*—The Constitution and Regulations of the Glasgow and Statistical Society; and part first of the transactions of that Society. (Glasgow, 1836.) Presented by James Cleland, Esq. LL. D. Glasgow, President of that Society.

6. The reports of the following committees were read and approved of:—

1. On Mr Young's voltaic battery.
2. On Mr Kemp's and Mr Rankine's railroad switches. Mr Dunn, Convener.
3. On Mr Roxbey's improved kitchen lamp.
4. On Mr Weir's dome arch.
5. On Mr Lothian's equable flow-cock.
6. On Lieutenant A. M. Skene's paddle-wheel.
7. On Mr Brown's mucilage of the Fuci.
8. On Mr M'Farlan's prevention of explosion in boilers.
9. On the Rev. Mr Tough's Orrery.
10. On Mr D. Macgregor's inside escapement.
11. On Mr Milne's method of giving motion to a shaft lying at an angle.

7. The report of the committee on Mr R. Wilson's improved Vice—(though ready and very favourable)—not being given in, was remitted to the prize committee.

8. The committee on Mr Kemp's and Mr Rankine's railroad switches, no

being ready to report the result of trials as to their relative results, was continued.

Mr John Eager, teacher of music, 48 North Fredrick Street, was admitted an ordinary member.

The following gentlemen were proposed by the Council and elected honorary members, viz:

M. I. Brunel, Esq. F.R.S., civil engineer, London; the Rev. Lant. Carpenter, LL.D., Bristol; James Cleland, Esq. LL.D. Glasgow, President of the Glasgow and Clydesdale Statistical Society, &c. &c.; John Alston, Esq. of Rosemount, Patron and Treasurer of the Asylum for the Industrious Blind, Glasgow.

The Council reported concerning apartments in Hanover Street, which have been taken as a Museum, for the accommodation of the models belonging to the Society, and for meetings of committees.

The Society held its first Meeting for this Session, in the Royal Institution, on Wednesday 15th November 1837, at 8 o'clock P.M.—Sir John Graham Dalyell, on taking the Chair. as one of the Vice-Presidents, adverted to the objects of the Society, and remarked—

That during the preceding sixteen sessions, since its institution, probably none other had diffused equal benefits. Contrasting the difficulties which formerly opposed communications of the works of the ingenious, with their present advantages, they had only now to submit their projects, or their inventions, to the Society, to be ensured of patronage. But he desired particularly to enforce, that it was not perfect and completed works alone which were desirable. The progress of the arts and sciences was slow,—composed of many parts,—the offspring of many inventors and improvers; so that even the slightest hints and suggestions might prove of infinite utility. Nowhere was this better exemplified than in the slender commencements of a watch, the most delicate—or in the origin of the steam-engine, the most powerful—of machines. The Society embraced many different objects, of which a fair and promising specimen was afforded by the billet of the evening. Most interesting facts were continually emerging concerning the rapidity of traversing the country; but now it appeared as if a new epoch in history were about to open, in the instantaneous communication of intelligence by physical and mechanical properties ingeniously combined.

The following communications were then laid before the Society:

1. Essay on the forms of the Teeth of Wheels:—Part I. Wheels with the Axes parallel to each other. By Edward Sang, Esq., F.R.S.E., M.S.A.—drawings were exhibited.

2. Drawing and verbal Description of an improved vertical Watch. By Mr Thomas Robertson, watchmaker, Rothesay.—The Watch was exhibited.

3. Drawing and verbal description of an improved Lever Watch. By the same.

4. Drawing and verbal description of an improved Escapement for a Chronometer. By the same.

5. Model of a machine and apparatus for conveying intelligence, for making communications with great rapidity between points or places at a distance from each other, by means of Electric or Voltaic currents and Magnetism, was exhibited in action; and a description of said machine and apparatus, with remarks on its utility and national importance, were read. By William Alexander, Esq. 19 Windsor Street, Edinburgh.—At the request of the Vice-President, the machine rapidly communicated the words "Queen Victoria," "Society of Arts," "Alexander."

6. *Donation.*—Descrizione di Alcuni Instrumenti da Misurare gli Angoli per Riflessione, Memoria. Del Professor Gio. Battista Amici, Astronomo dell' J. R. Museo di Firense,—Socio della Societa' delle Arti di Scozia, &c. (Printed at Modena, 1836.) From the author.

The following candidates were admitted as ordinary members, viz:

Walter Jollie, Esq. W.S. 8 Albany Street; William Alexander, W.S. 19 Windsor Street; Mr James Ruthven, architect, 33 Montagu Street.

The Annual General Meeting of the Society was held in the Royal Institution, on Wednesday the 6th December 1837, at eight o'clock P.M. Sir John Graham Dalyell, V. P. in the Chair.

The minutes of last meeting having been read and approved of, a dutiful and loyal address to her Most Gracious Majesty Queen Victoria, on her accession to the Throne, which had been prepared by the Council, was unanimously adopted by the Society. Thereafter the following communications were read and exhibited :—

1. On an extraordinary instance of Refraction. By William Galbraith, Esq. Teacher of Mathematics, Edinburgh, M.S.A.—The thanks of the Society were given for this Paper, which was ordered to be printed in the Transactions.

2. Letters on the subject of the Society's Report on the best Alphabet and Mode of Printing for the Blind, received from the following gentlemen, were read, viz :—The Rev. William Taylor of York, Hon. M.S. Arts; the Rev. Dr Lant Carpenter, of Bristol, Hon. M.S. Arts; The Rev. Thomas Bold, of Liverpool School for the Blind.—Thanks were given to these gentlemen.

3. Donation. A Ky-ee-lee or Boomerang, being a missile weapon used by the natives of Australia, possessing very remarkable properties. From Mr Gunn, of the Dublin Advertiser.—Thanks given to Mr Gunn.

4. Specimens were exhibited of various articles manufactured from Caoutchouc, under Letters Patent, such as Elastic Web for Saddlegirths, Rollers, Surgical Bandages,—Galashes, Sheet Rubber, Elastic Tubing,—Cables,—and Towing-ropes,—Window-sash and Window-blind Lines,—Ropes and

Bands for Coal mines, Bands, flat and round for Machinery,—Varnishes,—Paint for Outside-work, &c. &c.,—selected in London, by Mr James Dowie, boot and shoemaker, 57 Frederick Street, Edinburgh, M.S.A.—Thanks voted.

5. The Anemometer, invented by Mr R. Adie of Liverpool, whereby the most minute Changes in the Force or Velocity of the Wind, or current of Air, may be measured,—for which the Silver Medal was awarded in 1836,—was exhibited through the kindness of Robert Stevenson, Esq.—After some interesting observations on the subject of Anemometers, the thanks of the Society were voted to Mr Stevenson.

6. The List of Prizes offered by the Society for the current Session 1837–38, was distributed.

7. The Models, Drawings, &c. of Inventions, &c. (Session 1836–37) for which Prizes have been awarded, were exhibited.

8. The Report of the Prize Committee awarding the Prizes for Session 1836–37 was read, and the Prizes were delivered to the successful Candidates as follows:—

1. To Mr James Milne, brassfounder, Edinburgh, C.S.A., for his Model, Drawings, and Description, of a New Method of communicating Motion to a Shaft lying at an Angle,—read and exhibited 16th April 1837,—the Society's Honorary Silver Medal.

2. To George William Hay, Esq., 2 Duncan Street, Newington, Edinburgh, for his Model and Description of an Improved Turnip Cutter,—read and exhibited 23d November 1836,—the Society's Honorary Silver Medal.

3. To Mr James Gall junior, printer, Edinburgh, M.S.A., for his Specimens of Printing on *both* sides of the Paper for the Use of the Blind, with remarks on its advantages, &c.,—read and exhibited 10th May 1837,—the Society's Honorary Silver Medal.

4. To Mr J. B. Mould, engraver, Edinburgh, M.S.A., for his Tuning Fork, with a Scale engraved on it, so that any note may be struck at pleasure,—exhibited 10th May 1837,—the Society's Honorary Silver Medal.

5. To William Galbraith, Esq., teacher of mathematics, Edinburgh, M.S.A., for his Paper on the Erroneous Geographical Position of several Points in the Firth of Clyde,—read 7th December 1836; which tended materially to expedite the order for the resumption of the Trigonometrical Survey of Scotland,—the Society's Gold Medal, value Ten Sovereigns.

6. To Mrs Brydie, teacher in the Academy at Dollar, for her Specimens of Flowers modelled in Wax,—exhibited 12th April 1837,—the Society's Silver Medal, value Eight Sovereigns.

7. To Mr Andrew Monro, shipowner, Limekilns, for his Model and Description of a Machine intended to be used as a ship's Windlass,— read and exhibited 15th February 1837,—the Society's Silver Medal, value Five Sovereigns.

8. To Mr Robert Wilson, engineer, Paul's Work, Edinburgh, for his Model and Description of an Improvement on the Common Vice and Vice-Chuck,—read and exhibited 15th February 1837,—the Society's Silver Medal, value Five Sovereigns.

9 To Mr James Young, chemical assistant in the Andersonian University, Glasgow, for his Account of a New Voltaic Battery, being a modification of

the construction recommended by Mr Faraday, with a model,—read and exhibited 15th March 1837,—the Society's Silver Medal, value Five Sovereigns.

10. To Mr John Lothian, 21. Athole Cresent, Edinburgh, for his Model and Description of an Equable Flow-cock for bottling Liquors, without disturbing the Sediment in the Cask, and without loss of time,—read and exhibited 10th May 1837,—the Society's Silver Medal, value Five Sovereigns.

11. To the Companies of Messrs Maclure and Macdonald, and of Messrs Allan and Ferguson, lithographers, Glasgow, for their respective Specimens of Lithographic Drawing and Printing, from transfer Drawings,—exhibited 24th May 1837,—in respect that these Specimens offered in competition are judged to be equal in merit,—the Society's Silver Medal, value Eight Sovereigns, to each of the Companies.

12. To the Companies of Messrs Allan and Ferguson, lithographers, Glasgow, for their Specimens of Lithographic Drawing and Printing, from Chalk Drawings,—the Society's Silver Medal, value Eight Sovereigns.

13. To Mr Thomas White, wright, Straiton, county of Edinburgh, for his Model and Description of a Snow-Plough, for clearing new-fallen snow from foot pavements in Town, and from the walks in pleasure-grounds,—read and exhibited 7th December 1836,—Two Sovereigns.

The Committee reported, that there had been no competition for the following prizes :—1st Prize, Society's Gold Medal of 25 Sovereigns ; 2d ditto, ditto, 18 ditto.

Besides the thanks acknowledged to be due to all those Gentlemen who have sent communications to the Society, the special thanks of the Society were given to the following Gentlemen for their respective communications, viz. :—

1. To Sir George Mackenzie of Coull, Baronet, F.R.S.E., M.S.A., for his Model and Description of an Annular Fountain Table Lamp (Argand burner) calculated for being used with Cocoa-nut or other concrete Oils ;—read and exhibited 23d November 1836. *

2. To Mr Samuel Brown, jun., Haddington, for his paper on the Mucilage of the Fuci, with remarks on its application to the Useful arts ;—read 26th April 1837.

3. To Edward Sang, Esq., F.R.S.E., C.S.A., for his various Communications, and the general interest he has taken in the Society's proceedings during the past Session.

4. To Mungo Ponton, Esq., W.S., F.R.S.E., M.S.A., for a Paper on the Advantage of increasing the Adhesion of Wheels of Locomotive Carriages to the Rails ; and on a method of increasing that Adhesion, and also of preventing the Wheels from Starting off the Rails ; with Drawing and Model ;—read and exhibited 24th May 1837.

The Society's Gold Medal, value Twenty Sovereigns, lately awarded for the best Alphabet and best Method of Printing for the Use of the Blind, was delivered to the heirs of the late Dr Edmund Fry of London.

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* Since this Lamp was submitted to the Society, Sir George Mackenzie has succeeded in burning Cocoa Oil in the *Common* Lamp.

The following candidates were duly admitted as ordinary members, viz. :—

J. G. Hopkirk, Esq., LL.B. W.S., 75, Great King Street; Gilbert Laurie Finlay, Esq., W.S., 130 George Street.

In terms of Law XI. the Society elected the following office-bearers for the ensuing year :

President, Sir John Graham Dalyell, Kt., F.A.S.—*Vice-Presidents*, David B. Reid, M.D., F.R.S.E.; Mungo Ponton, W.S., F.R.S.E.—*Secretary*, James Tod.—*Foreign Secretary*, Alexander Adie, F.R.S.E.—*Treasurer*, G. Marjoribanks.—*Curator of Museum*, John Dunn, and twelve ordinary Counsellors.

In terms of Law XIII. the Society appointed a Committee to audit the Treasurer's books, and to report thereupon, and generally on the state of the funds of the Society, at next meeting. The books were laid on the table.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Storm at Constantinople.*—Extract from a letter from Admiral Roussin to M. Arago regarding certain periodical storms at Constantinople.—" Last night (the 10th August) we had a violent storm; it is periodical, and occurs almost every year between the 10th and the 15th August. This storm commenced in the south, and then veered to the north, approaching the Black Sea, its ordinary domain. It lasted from one o'clock in the morning to daybreak with great violence, and accompanied by a furious rain; the lightning struck three points near to one another; at Pera, at the embassy of Denmark, and at that of Spain it broke the doors, burnt the carpets and curtains, but without touching the panes of glass. The other stroke fell on a small Greek vessel, broke its mast, killed a man, and wounded another. At other times storms are by no means frequent here; I have never seen more than three or four in a year."

2. *Great Meteor observed at Paris on the* 21*st September* 1837. (Note communicated to the Academy by M. Mauvais, pupil at the Observatory.)—" On Thursday 21st September 1837, at 7h. 48′ mean time, as I was crossing the square of the Pantheon, on my way to the Observatory, I suddenly perceived a dazzling ball, which produced so great a light *that objects threw a distinct shadow.* It commenced its course from a point situated at nearly an equal

distance from the eagle and the dolphin; passing θ of the Eagle, it was suddenly extinguished near α Capricorn, a little to the east of that star, leaving behind it a long luminous train. The duration was not above 6″ or 7″; nevertheless, as the light which it diffused made me turn my eyes to the quarter from which it came, it is probable that the meteor had existed for some seconds before I perceived it. Its form was rounded at the lower part (that which, following the movement of the ball, was in front); the upper part was less distinctly terminated. It gave out in all directions brilliant rays of white light. Its diameter, in a horizontal direction, seemed to me equal to a fourth part of that of the moon. The sudden appearance of a light so vivid, in a sky which was quite obscure (the moon had not then risen), called forth an exclamation of surprize from all those near me who were witnesses of the phenomenon."

3. *Aurora Borealis.*—In terminating a communication made to the Petersburgh Academy, regarding the aurora of the 18th October 1836, as seen at Dorpat, MM. Struve and W. Preuss deduce the following general conclusions:—"The greyish segment placed near the northern point of the horizon, which seemed to serve as the base of the whole aurora, and which we perceived for a long time at Dorpat, is only the darkened colour of the sky and not a cloud. We have often observed this segment in very obscure nights and at considerable heights above the horizon, and we have then constantly seen the stars without any sensible diminution of their light. Its obscurity is only the consequence of the contrast which exists between it and the luminous arches which surround it. When the segment is in part divided and illuminated by the luminous arches, we must attribute this effect to the existence of the luminous matter in the parts of the sky which did not previously exhibit any brightness. 2. It appears to us very probable that the distance of the aurora from the observer is not very considerable, at least when the aurora is vividly extended and intense; and that the position of the light is in the clouds. We may even conjecture that the phenomenon of the polar light is partly to be attributed to the constitution of the atmosphere in the region of the clouds. The reflection of the aurora on the whole surface of the ascensional cloud, is explained by the short distance between the earth and

the red light that is perceived; for the simultaneous existence of the aurora borealis with the formation of the cloud, proves the great similarity of the white bands and the white veil to those of the same sort which present themselves without any aurora, but which are much less intense; so that, when we perceive the white horizontal bands that have lasted a long time, we believe, first of all, that they are a white cloud formed by the debris of the first aurora borealis. 3. We are of opinion that the explanation of the aurora requires the observation of general terrestrial circumstances as well as of local conditions, especially when the phenomenon is of great extent.

GEOLOGY.

4. *The Lartington-Hall Museum of Natural History.*—During the last few years we have witnessed, with much satisfaction, the commencement of collections of natural history in many of our smaller provincial towns; and we now notice with pleasure the recent erection of a private museum at Lartington Hall in Yorkshire, the seat of Henry T. Maire Witham, Esq. F. R. S. E. The apartment in which Mr Witham's extensive collections have been placed is equally remarkable for the elegance and good taste of its architectural design and decorations, and for its adaptation to the purposes of scientific arrangement and study. Besides containing valuable mineralogical and conchological collections, the Lartington museum is particularly rich in fossil organic remains, and we allude more especially to the fossil fishes of this and other countries, and to the splendid saurian animals from Whitby and Lyme Regis. The geological department includes a good general suite of mountain rocks, and extensive series of specimens from Germany and Italy; but it derives its chief interest from the local collections from many of the counties of Scotland, which are arranged geographically, and are the products of Mr Witham's personal activity and zeal during his numerous geological tours through the mainland and islands. We have to express the hope that Mr Witham's example in forming a museum, and his liberality in throwing it open to strangers, will be speedily followed by many noblemen and gentlemen in other parts of the kingdom.

5. *On the Increase of Temperature in the interior of the Earth.* —M. Arago has lately communicated to the Academy of Sciences the result of the thermometrical observations which he made on the first of May last in the well which is now being sunk at the slaughter-house (Abattoir) of Grenelle. The boring has now reached the depth of 1312 English feet. The bed of chalk, in which they have for so long been engaged, is not yet traversed, but the numerous flints, which were unceasingly met with at lesser depths, have now disappeared. The city of Paris has determined that the boring shall continue to the depth of 2295 English feet, if the spouting water be not found sooner. It is presumed that the water which will issue from so great a depth will possess a temperature of between 93°2 and 95° Fahr., and, in that case, it might be employed for hot-baths, &c. But however this may be, we shall now adduce the thermometrical observations which have been made at the depth of 1312 English feet. On the 29th of April, at 7 P. M., four instruments were sent down, viz. two of M. Bunten's self-registering thermometers, one a *diversement* thermometer which M. Magus of Berlin had recently sent to M. Dulong, and another of the same construction manufactured by M. Walferdin. The two first were contained in a copper tube, in which they were secure from the pressure of water; the third was open at the top, but in such a manner that the pressure could not alter its form; and the fourth was enclosed in a glass tube, which was hermetically sealed. These four instruments, after having remained for about thirty-six hours in the well or bore, were removed from it on the first of May about 7 A. M.; they then indicated the following temperatures:—

The first thermometrographe of M. Buten, . .	74°.3
The second, do. . .	74°.21
M. Magus' thermometer, a diversement, . .	74°.30
M. Walferdin's do.	74°.66

Assuming, then, 74°.3 Fahr. as the temperature at the depth of 1312 English feet, if you subtract from this number that of 51°.08 Fahr., which indicates the mean temperature of the surface of the earth at Paris, 23°.22 will remain for the increase of temperature, corresponding to 1312 English feet of depth, or what comes to the same thing, 1°.8 Fah. for 101.2 English feet. If we take the case of the observatory as the starting point for the temperature where it is at 53°.06 Fah., 21°.24 Fah. will then be given as the augmentation for 1222.5 English feet, which corresponds to 103.348 English feet for each centigrade degree.

6. *Effects of Clearing a Country.*—As new facts proving the influence of clearing a country, in lowering the temperature and diminishing the streams of water, M. Devize de Chabriol communicated to the Academy of Sciences a notice, in which he established that, according to old charters of the 13th and 14th centuries, the slope of the hill of Saint Flour was at that time under vine cultivation; he adds that now-a-days its cultivation no longer proceeds. The chestnut, also, has now disappeared from many districts in which it formerly flourished. Hence numbers of villages which were situated near the summit of the mountains have been abandoned, and many old springs have also dried up.

7. *Fossil Bones of Sansan.*—In Vol. xxii. p. 403 of this Journal, we inserted a notice regarding the fossil bones of the tertiary formation of Simorre, Sansan, &c. More recently a large collection of the organic remains of the Sansan deposit has been sent to Paris by M. Lartet; and, from the report made on the subject to the Academy of Sciences by M. de Blainville, we now extract the following summary of the animals hitherto found at this highly interesting locality. *Class Mammalia*—Monkeys belonging to a group analogous to the Gibbons, but not identical with the true Gibbons: of the insectivorous carnivora the genus Talpa; of the plantigrade carnivora the genus Ursus and another genus resembling it; of the digitigrade carnivora the genera Felis, Viverra, and a new genus Amphicyon; there is one fragment of a lower jaw which seems to indicate an animal of the Seal family; of the terrestrial edentata, a genus allied to the Orycteropus; of the order Glires, the genera Myoxus and Lepus; of the elephants the genus Mastodon; of the lamantines, the genus Dinotherium; of the pachydermata, the genera Rhinoceros, Palæotherium, Equus, Sus, and Anoplotherium; of the ruminantia, the genera Cervus and Antilope. *Class Aves*—Two species of a genus not yet determined. *Class Reptilia*

—A species of Emys, and some species of Coluber. *Class Amphibia*—A species of the genus Rana. Of the *Mollusca* there have been found some planorbes, helices, and a bivalve, which bears a great resemblance to the Unio margaritifera of Lamarck.

8. *Mud Volcanos.*—In the proceedings of the Geological Society of France, it is mentioned that M. De Verneuil had visited the peninsula of Caman, and the eastern portion of the Crimea where the small village of Kertsch is situated, a district covered by hills which owe their origin to muddy eruptions, generally accompanied by springs of naphtha and by springs of muddy water which evolve gas; they have a height of from 200 to 300 feet above the level of the plains. Their flanks are for the most part furrowed by deep crevices produced by the action of the waters of the atmosphere on substances having but little solidity. At the summit there occur small cavities, from an inch to three feet in depth, and having the form of craters, which are constantly bringing to the surface a small quantity of muddy water. Gas is frequently disengaged from this water; and sometimes on the continuation of the hill there occur springs of bitumen, which are so abundant, that M. de Verneuil has counted forty points whence naphtha was obtained in buckets like water. An eruption, which lasted six hours, had been preceded during three days by subterranean reports resembling discharges of artillery. Portions of black earth, which assumed a variety of forms, were projected to a height of from five to six toises. A smoke, or rather a gas, having a bituminous and sulphurous odour, was uninterruptedly emitted, and at intervals jets of flame were perceived. At the distance of a few toises, the earth seemed to move under the feet of the spectator.

9. *Morphology of Rocks.*—Dr Jackson of Boston states in a letter read to the Geological Society of France, that the results of his observations on the chains of mountains and the direction of their dislocations, confirm perfectly the theory of M. Elie de Beaumont. He has noticed some very curious and beautiful examples of a passage from new red sandstone to hornstone porphyry and ribbon jasper. The same geologist is in possession of some interesting specimens confirming the conversion of magnesian limestone into true dolomite by the action of trap-rocks; and

others of a manganesian crystalline limestone produced by the same cause, and in which the carbonate of manganese has completely replaced the magnesia of the magnesian limestone, a kind of isomorphism which Dr J. had not previously seen. Near Boston, a mine of anthracite belonging to the greywacke formation is traversed by a trap dyke, which has changed the coal at the line of contact into a true graphite.

10. *Height of Waves.*—Mr Pentland writes to M. Arago, that in the latitude of Cape Horn, during the most violent tempests to which the Stag frigate was exposed, the waves never rose to a greater height than twenty English feet above the mean level of the sea. The greatest height of the waves above the deck of the vessel was eighteen English feet.

11. *Gas of the Sacred Fires of Bakou, near the Caspian Sea.*—Several abundant natural sources of combustible gas are known, such as those of the Apennines, and the gas which supplies the gasometer of Fredonia (State of New York), in the neighbourhood of Lake Erie. But the most remarkable exhalations of gas, both on account of the quantity of gas evolved, and the reputation which they enjoy among the nations of the East, are, without doubt, those producing what are called the sacred fires of Bakou. No account has hitherto been given of the chemical nature of this gas. M. Hess of Petersburgh having received from M. Lenz a certain quantity carefully preserved in bottles, analyzed it, and ascertained that it is composed of carburetted hydrogen gas, mixed with a little of the vapour of naphtha, and that 100 parts contain—Carbon, 77.5; Hydrogen, 22.5 = 100.0. The presence of 24.6 of hydrogen in the 100 parts, would be necessary to make the mixture correspond with the formula $C\,H^4$; there is, therefore, a small excess of carbon derived from a minute quantity of carbonic acid. M. Hess ascertained that there is no admixture of olefiant gas, which is so much the more remarkable, because naphtha itself is liquid bicarburetted hydrogen, and there is every reason to believe that olefiant gas cannot be transformed by heat into carburetted hydrogen.

12. *Volcano of Aconcagua in Chili.*—Mr Pentland has addressed a letter to M. Arago, dated the 8th July 1837, in which he states, that according to several observations he had made, the northern peak of the mountain of Aconcagua, the most ele-

vated point of the Cordillera of Chili, is placed 1° 41′ to the east of Valparaiso, and that its latitude is 32° 38′. The angle of height of this peak, measured by the theodolite, is 1° 55′ 58″; from which Mr Pentland deduces 7300 metres as the absolute height of Aconcagua above the level of the sea. The observations of Captains Beechey and Fitzroy, assigned a height of sixty metres less than the result now given. These determinations tend still farther to lower the rank formerly held by Chimberazo. The following is a comparative view of three other heights with that of Aconcagua :—

Himalayah, ...	7821 mètres
Nevado de Sorata (Upper Peru), . . .	7696 ...
Aconcagua (Chili), ...	7300 ...
Chimberazo (Peru), ...	6530 ...

MINERALOGY.

13. *On Intermediate Chemical Combinations which have not yet been detected by chemists.*—M. Biot has very lately read a paper, one of the objects of which is to prove, that, besides the definite and limited combinations between bodies recognised by chemists, there are other intermediate combinations which they cannot detect, and whose existence is revealed by optical processes. It is thus that the continual and successive changes which polarized light suffers in passing through a solution of tartaric acid in water indicate, at each addition of water, states of combination at present unknown to chemistry; and further, it is thus that sulphuric acid and soda, of which only two degrees of combination are admitted, very probably produce a multitude of other intermediate combinations.

14. *Tantalate of Iron and of Manganese.*—M. Desplaces has discovered at Charmasse, near Autun, some specimens of the tantalate of iron and of manganese. This mineral occurs in the quartz which contain beryls, garnet, &c.; is found in small black masses; has a metalloidal aspect; and a fracture somewhat lamellar.

15. *Artificial Rubies.*—M. Gaudin, as stated in the Comptes Rendus, August 1837, has lately announced to the French Academy of Sciences, that he has been able, by the following process, to make rubies, having the characters of the precious ruby. In order to obtain these artificial rubies, M. Gaudin uses a pla-

tinum blowpipe of a single piece, formed of two hollow concentric cylinders, communicating each by one of the extremities, the one with a reservoir of hydrogen, the other with a reservoir of oxygen; while the two other extremities are pierced with convergent openings, destined to effect the mixture of the gases. It is well known that alumina is fusible with the oxygen and hydrogen blowpipe; but no one before M. Gaudin had endeavoured to melt this earth into globules several millimetres in size. Having submitted a portion of potash alum to the action of his blowpipe, he obtained a perfectly round and limpid globule. The platinum tube being perforated and melted at several places, he obtained, after the cooling, instead of a limpid spheroid, an opaque elongated globule, and covered internally with crystals which may be referred to the rhombohedron or to the cube. *These crystals scratch rock-crystal, garnet, topaz, and spinel; with regard to hardness, therefore, they agree with the oriental ruby.* They appear to be composed solely of alumina, the potash volatilizing at the high temperature to which the alum is submitted.—Having obtained an apparatus stronger than the one first used, he made an experiment with some ammoniacal alum mixed with from four to five thousandths of chromate of potash; the whole being previously calcined, he gave it the form of a spherical cup, in order to obtain a maximum effect, by directing the flame to the concave part. In a few moments the inner surface of the cup was covered with globules of a beautiful ruby-red colour, faintly translucent, and some of which exhibited the form and cleavage of the ruby. M. Malaguti, who had occasion to analyse these globules, found them to be composed of ninety-seven parts alumina, one oxide of chrome, and two parts of silica and lime; which composition is analogous to that of the ruby.

BOTANY.

16. *Remarks on Dr Wight's paper regarding the Tree which produces the Gamboge of Commerce*, at p. 109 of this Number were written in June last, with a view to their insertion in the then forthcoming Number of this Journal. I am glad that there has not been room for them in an earlier Number than the present, because I have since received additional, and what seems

satisfactory information regarding the claim of *Hebradendron gambogioides* to be considered a native of Ceylon. In a letter which I have from Mrs Walker, dated 1st and 2d May 1837, she mentions a tour she had made through different parts of the island, and adds, " we found the Ceylon gamboge-tree several times in forests distant from the habitation of man, which proves the tree to be indigenous." She afterwards adds, however, " The tree does not abound much." In my account of this tree, in the Companion to the Botanical Magazine, I could only describe the male flower and the fruit, and I stated that Mrs Walker's account seemed to shew that the plant was monœcious. I had no specimen of female blossom. I have now the satisfaction of adding, that Mrs Walker, in her last letter, assures me she has ascertained the tree to be diœcious, that the inflorescence of the female tree is similar to that of the male, the flower white and a little larger, and she has most kindly sent me a sketch of this, which presents a germen precisely in miniature of the fruit, and surrounded (like it) with several (ten ?) abortive stamens, which seem united at the base.—*Dr Graham.*

17. *Discovery of* Carex Leporina, Linn., *near the Summit of Loch-na-Gar.*—I received a few weeks ago from my friend Dr Murray of Aberdeen, a specimen of a *carex* gathered by Mr Dickie in August 1836, on rocks near the summit of Loch-na-Gar, towards the west side of the mountain, which appears, upon careful examination, to be *Carex leporina*, Linn., Willd., and Flor. Dan., *C. lagopina*, Wahl., and *C. Lachenalii*, Schkuhr, a species never before found in Britain.—Dr Greville has compared the Scotch plant with Lapland specimens in his herbarium, sent to him by Fries, as well as by the Unio Itineraria, and he finds that it agrees with them in every respect. It is an alpine species, having some resemblance to *C. ovalis*, Gooden, but is only about one-half the size of that species. The following are the characters given by Wahlenberg: spiculis tribus basi masculis, confertis, subglobosis, squamis capsula brevioribus, capsulis subrotundis, acutis, obtuse-marginatis, ore subintegris. The plant is frequent on the Alps of Lapland in cold moist situations. The culm is triquetrous and acute-angled, but always smooth, the spikelets, more especially the lower ones, are often somewhat

oblong, but the capsules, when fully ripe, become so open that the spikelets become subglobose. The scales are small, shorter than the capsule and somewhat obtuse; the capsules are acute, of a tawny or brownish colour, the margins being somewhat obtuse and quite entire. The *Carex leporina*, Huds., Leers, Lightf., Ehrh., Wahl., is merely a synonyme of *C. ovalis*, Gooden.—*Letter, dated 12th December* 1837, *from Dr Balfour to the Editor, Professor Jameson.*

18. *Macquart and Hope's Experiments on the Colours of Plants.*—Our readers are informed, in answer to a query, that at pp. 429 and 430 of the twentieth volume of this Journal, will be found an abstract of the results of Dr Macquart's experiments, as given by himself, in his work entitled "*Die Farben der Blüthen;*" and in our twenty-first volume, pp. 315, 316, and 317, a notice of Dr Hope's memoir, entitled "*Observations and Experiments on the Coloured and Colourable Matters in the Leaves and Flowers of Plants, particularly in reference to the principles upon which Acids and Alkalies act in producing Red, and Yellow or Green.*"

19. *On the Floating Masses of Fucus occurring near the Cape Verd Islands.*—M. Kunth lately presented to the Academy of Sciences of Paris, in the name of M. Meyen of Berlin, a specimen of the *Sargassum natans* (Fucus natans, Linn.), brought from the celebrated *Mar de Sargasso*, near the Cape Verd Islands. M. Kunth remarked, that this individual, like all the others observed by M. Meyen in these latitudes, does not present the slightest trace of a point of attachment. It was therefore never attached to rocks or to any other supporting body at any period of its growth, but must have been developed floating on the surface of the sea. The opinion generally adopted by voyagers, that these plants have been torn from their original situations by the waves, and collected by currents in the Mar de Saragossa, appears to M. Meyen to be inadmissible; and he is inclined to believe that they have been produced at the place where they are observed. The same naturalist maintains that such individuals formed at the surface of the water never exhibit fructification.

ZOOLOGY.

20. *Ehrenberg's Discovery of Stomachs in the Baccillariæ.* Meyen and other well known naturalists refer the Baccillariæ to the vegetable kingdom, because no one had hitherto observed them to take in any coloured substance, like the other infusoria. Recently, however, Ehrenberg has succeeded in feeding various species belonging to the genera, *Navicula*, *Gomphonema*, *Arthrodesmus*, and *Closterium*. Vesicular ventral sacs, similar to those of the other polygastrical infusoria, were detected. "I could count," says that learned naturalist Wiegmann, "with the greatest distinctness, from six to seven ventral sacs completely filled with blue colouring matter, in the clear middle part of a *Navicula gracialis*, exhibited by Ehrenberg. With this discovery, therefore, the most perfect proof has been afforded of their coincidence with the Polygastrica, which, however, the peculiar gliding motion that was perceptible in many of them had before sufficiently indicated; which motion cannot be compared with the lively motions of the sporules of Algæ, or with the motions of Oscillatoria.

21. *Experiments on the Plague.*—Admiral Roussin writes to M. Arago: "You will doubtless hear of the heroic Dr Bulard, who has studied the plague at Smyrna in the most courageous manner. He slept in the same beds with the diseased, and wore their clothes while they were yet warm; he also inoculated himself with the matter of the sores; and all this with impunity, although two criminals exposed to the same experiments died, the one in seven, and the other in five days. I must inform you, at the same time, that this daring observer is of opinion that the plague is almost always contagious by contact."

22. *White Phosphorescent Tract of Sea in the Persian Gulf.*—William Newham, Esq., read to the Royal Asiatic Society of London an extract of a letter from a naval officer who sailed on board the sloop Clive, from Bombay to the Persian Gulf, in 1832, detailing a curious appearance of the sea. In the month of August, about eight at night, while the ship was rapidly advancing with a high sea, they were suddenly surrounded with water as white as milk. The colour near the ship was of a dead white. The water, when taken up in a bucket, did not differ from ordinary sea water. On agitating it, it became

phosphorescent, but not more so than usual. The vessel sailed about fifteen miles through this white sea. Colonel Briggs remarked upon this letter, that he had himself seen a similar phenomenon on board the Benares, in the year 1810; and he understood that the appearance had been many times witnessed in these latitudes.

NECROLOGY.

23. *Death of Dr Meredith Gairdner.*—By a late arrival from North America, we deeply lament to learn the death of a highly accomplished and talented naturalist, Dr Meredith Gairdner of this city, author of an excellent and well known work, entitled "The Natural History, Origin, Composition, and Medicinal Effects of Mineral and Thermal Springs," and also of many valuable contributions to the Philosophical Journals. Some time ago he left the Columbia River for the Sandwich Islands, trusting that the disease under which he was suffering might be alleviated, or even entirely removed, by the genial climate of that delightful region. But his hopes were vain, he gradually sunk under the attack, and expired on the 26th of March last. His numerous manuscripts and collections have not yet reached this country.

NEW PUBLICATION.

History of British Quadrupeds, including the Cetacea. By Thomas Bell, Esq. F.R.S. &c. One volume octavo, with numerous plates. John Van Voorst, London.

We are happy to announce the completion of this beautiful and valuable work. Like all the other volumes illustrative of the Zoology of Great Britain, in the course of publication by M. Van Voorst, it is distinguished by amplitude and accuracy of detail, agreeable style, and sound views of zoological science.

List of Patents granted in Scotland from 2d October to 20th December 1837, *inclusive.*

1. To Robert Smith, of Manchester, in the county of Lancaster, engineer, for an invention of "certain improvements in the means of connecting metallic plates for the construction of boilers, and other purposes."—2d October 1837.

2. To **George Whitmore**, of Austin Friars, in the city of London, merchant and underwriter, in consequence of a communication from a foreigner residing abroad, for an invention of "a new method of combining, by means of machinery and adhesive compositions, all kinds of fibrous materials, such as cotton, silk, flax, hemp, tow, fur, wool, hair, &c., into manufactured articles, which may be applied to the purposes for which paper, pasteboard, millboard, papier maché, parchment, vellum, leather, woven fabrics, felt, floorcloth, tarpaulin, and the skins of animals are used."—6th October 1837.

3. To **Thomas Clark**, Doctor of Medicine, Professor of Chemistry in Marischal College, Aberdeen, for an invention of "improved apparatus to be used in manufacturing sulphuric acid."—6th October 1837.

4. To **James Potter**, of Manchester, in the county of Lancaster, for an invention of "certain improvements in spinning machinery."—6th October 1837.

5. To **William Hearn**, of Southampton Street, Pentonville, in the parish of Saint James, Clerkenwell, and county of Middlesex, engineer, and **William Davies**, of Upper North Place, Gray's Inn Road, in the parish of Saint Pancras, and county of Middlesex, plumber, for an invention of "a certain improvement, or certain improvements in the construction of boilers for the generation of steam and heating water, or other fluids."—13th October 1837.

6. To **John Chanter**, of Earl Street, Blackfriars, in the city of London, and of Upper Stamford Street, in the county of Surrey, Esq., and **John Gray**, of Liverpool, in the county of Lancaster, engineer, for an invention of "improvements in furnaces, and apparatus connected therewith, for locomotive engines, and for other purposes."—18th October 1837.

7. To **Henri Quentin Tenneson**, late of Paris, in the kingdom of France, but now residing in Leicester Square, in the county of Middlesex, gentleman, in consequence of a communication from a foreigner residing abroad, for an invention of "an improved construction of the portable vessels used for containing portable gas, and of the apparatus or machinery used for compressing such gas therein, and of apparatus or mechanism for regulating the issue or supply of gas, either from a portable vessel, or from a fixed pipe communicating with an ordinary gasometer."—28th October 1837,

8. To **James Matley**, of the city of Paris, in the kingdom of France, and of Manchester, in the county of Lancaster, gentleman, for an invention of "a machine called a tiering machine, upon a new principle, for supplying colour to, and to be used by block printers, in the printing of cotton, linen, and woollen cloths, silks, paper, and other substances and articles to which block-printing is, or may be applied, without the aid or assistance of a person to tier upon."—4th November 1837.

9. To **Thomas Bell**, of South Shields, in the county of Durham, manufacturing chemist, for an invention of "improvements in the manufacture of sulphate of soda, which improvements, or parts thereof, are applicable to other purposes."—4th November 1837.

10. To **John Joseph Charles Sheridan**, of Ironmonger Lane, in the city of London, chemist, for an invention of "certain improvements in the several processes of saccharine, vinous, and acetous fermentation."—9th November 1837.

11. To **William Arthur**, of Glasgow, N. B., machine-maker, for an invention of "improvements in spinning hemp, flax, and other fibrous substances."—9th November 1837.

12. To **Baron Henry de Bode**, major-general in the Russian service, of Berner Street, Oxford Street, in the county of Middlesex, for an invention of "improvements in apparatus for retarding and stopping chain, or other cables or ropes on board ships or vessels."—20th November 1837.

13. To **Hamer Stansfeld**, of Leeds, in the county of York, merchant, for having imported an invention "for the application to certain machinery of a tappet and lever action, to produce a vertical or horizontal movement, through the medium of ropes or bands working over, under, or round pullies; as also of a new arrangement of mechanism, for throwing certain wheels in and out of gear," communicated to him by Christian William Schonherr, of Schneeberg, in the kingdom of Saxony.—20th November 1837.

14. To **Frederick Burt Zincke**, the younger, of Cranford Street, Mary-le-bone, in the county of Middlesex, Esq, for an invention of "the preparing or manufacturing of the leaf of a certain plant, so as to produce a fibrous substance, not hitherto used in manufactures, and its application to various useful purposes."—21st November 1837.

15. To **John Hanson**, of Huddersfield, in the county of York, leaden-pipe manufacturer, and Charles Hanson of the same place, watchmaker, for an invention of "certain improvements in machinery, or apparatus for making or manufacturing pipes, tubes, and various other articles, from metallic or other substances."—22d November 1837.

16. To **William Gilman**, of Bethnal Green, in the county of Middlesex, engineer, for an invention of "an improvement or improvements in steam-boilers and in engines, to be actuated by steam or other power."—22d November 1837.

17. To **John George Bodmer**, of Bolton-le-Moors, in the county of Lancaster, civil engineer, for an invention of "certain improvements in machinery for spinning and doubling cotton, wool, silk, flax, and other fibrous materials."—24th November 1837.

18. To **Richard Burch**, of Heywood, in the county of Lancaster, engineer, for an invention of "certain improvements in manufacturing gas from coal."—24th November 1837.

19. To **Moses Poole**, of the Patent Office, Lincoln's-Inn, in the county of Middlesex, gentleman, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "improvements in looms for weaving figured and ornamented fabrics."—30th November 1837.

20. To **Samuel Draper**, of Basford, in the county of Nottingham, lace-maker, for an invention of "certain improvements for producing ornamental lace or weavings."—30th November 1837.

21. To **Christopher Nickels**, of Guilford Street, Lambeth, in the county of Surrey, gentleman, for an invention of "improvements in embossing or impressing the surfaces of leather and other substances, applicable to various purposes."—30th November 1837.

22. To **Joseph Lockett**, of Manchester, in the county of Lancaster, engraver, in consequence of a communication made to him by a foreigner residing abroad, for an invention of "certain improvements in the art of print-

ing calicoes and other fabrics of cotton, silk, wool, paper, or linen, separately or intermixed."—1st December 1837.

23. To WILLIAM WILKINSON, of Lucas Street, in the parish of St George in the East, in the county of Middlesex, engineer, for an invention of "a certain improvement or certain improvements in the mechanism, or machinery by which steam-power is applied to give motion to ships or other floating vessels in or through water."—4th December 1837.

24. To HENRY BLUNDELL, of Hall, in the county of York, paint and colour manufacturer, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "an improved method of operating upon certain vegetable and animal substances, in the process of manufacturing candles therefrom, and the application of certain products resulting from this method to various useful purposes."—7th December 1837.

25. To JOHN HALL, of New Ratford, in the county of Nottingham, lace manufacturer, for an invention of "certain improvements in machinery, whereby cloth and other woven fabrics of various kinds may be extended, or stretched and dried in an extended state."—7th December 1837.

26. To JOHN UPTON, of Horsley Down Lane, in the county of Surrey, engineer, for an invention of "an improved method or methods of generating steam power, and applying the same to ploughing, harrowing, and other agricultural purposes, which method or methods is or are also applicable to other purposes to which the power of steam is or may be applied."—8th December 1837.

27. To WILLIAM HERAPATH, of the City of Bristol, in the county of Somerset, philosophical chemist, and James Fletcher Cox of the same place, tanner, for an invention of "certain improvement or improvements in the process of tanning."—8th December 1837.

28. To JOSHUA TAYLOR BEALE, of Church Lane, Whitechapel, in the county of Middlesex, engineer, for an invention of "certain improvements in, and additions to his former invention, known by the title of a lamp applicable to the burning of substances not hitherto usually burned in such vessels or apparatuses, and secured to him by letters-patent, bearing date at Westminster the fourth day of January, in the fourth year of the reign of his late Majesty King William the Fourth."—11th December 1837.

29. To WILLIAM FOTHERGILL COOKE, of Breed's Place, Hastings, in the county of Sussex, Esq., and Charles Wheatstone, of Conduit Street, Hanover Square, in the county of Middlesex, Esq. for an invention of "improvements in giving signals and sounding alarms at distant places by means of electric currents, transmitted through metallic circuits."—12th December 1837.

30. To JAMES LEONARD CLEMENT THOMAS, of Covent Garden, in the county of Middlesex, Esq. in consequence of a communication from a foreigner residing abroad, for an invention of "an improvement applicable to steam-engines and steam generators, having for its object economy of fuel."—20th December 1837.

31. To THOMAS JOYCE, of Clerkenwell New Road, in the county of Surrey, gardener, for an invention of "improved apparatus for heating churches, warehouses, shops, factories, hothouses, carriages, and other places requiring artificial heat and improved fuel, to be used therewith."—20th December 1837.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Remarks on the Scenery, Antiquities, Population, Agriculture, and Commerce of Central European Turkey. By Dr A. Boué. Communicated by the Author in a Letter to the Editor. (Concluded from page 131.)

Part 2.—*Population, Agriculture, and Commerce.*

The *Population* of the Turkish Empire seems to be greater than is generally supposed; but it is very difficult to obtain correct information concerning it, as the Turks only know the number of those men who pay the *Karatsch*, and are apt to regard any inquiries on the subject with anger or suspicion. In like manner, the Christians keep no register of their births and deaths, and only know the number of Christian families in any town or village. Besides, the Turks dislike to appear so few among so many Christians, and these latter are inclined to make the number of the Turks appear as small as possible. In Turkey they always talk of the number of houses, but this does not give sufficient data for ascertaining how many families there are; for very many persons have two or even three houses in the same court or garden, one of which is generally set apart for the use of strangers. It therefore becomes necessary to ascertain if each house contains a family, before we can think of calculating at the rate of a certain number, say four or five persons, for each house.

Notwithstanding these difficulties in forming a correct estimate of the population of Turkey, I am convinced that it is greater than t is stated to be by the latest writers on the subject; for I found many large and populous towns, and especially many large villages, on spots which are marked as desert places in the maps. I am quite aware that Turkish towns must appear larger than European ones of the same population, as the houses are generally only one story in height; but nevertheless I think we may estimate Salonichi at nearly 60,000 inhabitants, Monastir at 40,000 or even 50,000, Nisha at more than 10,000, Uskub at at least 10,000, Istip at nearly the same number, Castoria at 8000, Kostendil at fully that number, Karatova at 7000 or 8000, Perlepe and Djumaa at 5000 or 6000, Dubnitza and Ipek at 6000 or 7000, Komanova, Kalkandel, and Eski Palanka at 3000 or 4000, Pristina at 6000 or 7000, Gafadartzi at 2000 or 3000, Novibazar at 5000, &c.

The population of Wallachia is said to be greater than has been generally supposed. In Servia there are now at least 900,000 inhabitants, in Montenegro 100,000, and in Bosnia probably above a million. Upper Albania is less populous than Macedonia or Rumelia: in this last-mentioned country we find great numbers of Asiatic Turks collected together at different places. In the rest of European Turkey true Turks are rare, and, when met with, are generally found occupying situations under government. Mahometans from among the Albanians or Bosnian Servians are more numerous, but still form only a very small proportion of the population of these countries.

Most of the Christians belong to the Greek Church: the Roman Catholics, few in number, are scattered over Turkish Croatia, Herzegowina, and some parts of Albania. In the latter country we saw a remarkable instance of a marriage between a Catholic woman and an Albanian Mahometan. The Greeks disapproved highly of such a marriage, the more particularly so as they dislike the Catholics. The Servians, as well as the Bulgarians and Greeks, are much attached to their church, which is mainly to be attributed to the exertions and good character of the monks: indeed it is probable that, had it not been for them, the Christians would have become Mahometans, as has happened in Bosnia and some parts of Bulgaria. In the monasteries the people always find that holiness which they consider a necessary attribute of the priestly character; but it is not to be supposed that the ordinary Greek clergymen should exert any very salutary influence over their parishioners, from whom they are in no way distinguished, as they are allowed to marry, and are too often found keeping inns, or even working like common labourers. If they were properly remunerated, like the Protestant clergymen, they would probably be much more useful for the diffusion of knowledge, especially as many of them are at present very ignorant. On the other hand, I must say, in favour of the Greek clergy, that I nowhere found them hostile to instruction, as is the case with some of the Roman Catholic priests, particularly in the west of Europe. On the contrary, whenever they have sufficient intelligence to comprehend the advantage of diffusing knowledge among the people, they zealously establish schools, &c., where they have the means of doing so. We found no libraries anywhere: even the celebrated old one in the Rilo Monastery had been destroyed when the monastery was burned, the monks having probably preferred saving other things than the books.

The Wallachians also belong to the Greek church, but seem in many cases to be inclined to Catholicism. The like has happened in Hungary, where whole villages, and even a bishop near Grosswardein, are said to have joined the Catholic religion, on being allowed to retain some particular parts of the Greek ritual. It remains to be seen how far such proselytism may be carried on in Wallachia. The greatest anomaly in the Greek church in Turkey is to be found in Montenegro, where both the priests and the monks are soldiers or officers, and carry arms. There is, however, a sufficient excuse for this, owing to the cruel way in which the war is always carried on in that country between the Turks and Christians; as they are never at peace with each other, and both parties are constantly robbing and murdering their enemies whenever a favourable opportunity occurs. Besides, in Montenegro as in Albania, an injury inflicted on one of a family is sure to be avenged by the whole of his relations. It is therefore not surprising that the Turks, who like to live quietly, should form a very bad opinion of the people of Montenegro; although, at the same time, a Christian might travel pleasantly and safely through the country, meeting everywhere with hospitable entertainment. It sometimes also happens, in other parts of Turkey, that Turks, when obliged to travel alone through populous Christian countries, are exposed to ill-treatment, from which a Christian traveller would be exempted.

Since the Sultan began his reforms, many churches have been rebuilt in Turkey, permission to do so being generally pretty easily obtained on spots where churches formerly existed, although it is seldom allowed to build them on new sites. In Servia they have even again put up their bells, excepting in Uschitze, where the Mahometans, being pretty numerous, would not allow it. Elsewhere bells are only allowed to be used in the monasteries: at Rilo there are three pretty large ones. The Sultan must now continue his reforms, as any return to the former bad system of government would be most prejudicial; indeed since the emancipation of the Christians has been begun, it must be proceeded with, if he wishes to retain possession of this fine country and population.

It is very interesting to compare together the different inhabitants of European Turkey, such as the Servians, the Bulgarians, the Wallachians, the Greeks, and the Albanians. The Servians and Bulgarians may be said to be nearly the same people, and appear to be more numerous than the Greeks; although the latter extend beyond the kingdom of Greece into Thessaly, and even into Southern Macedonia, and are scattered in considerable numbers over all the maritime districts of European and Asiatic Turkey. However, the greatest part of Macedonia may be said to be a Bulgarian-Servian country; and even the Bulgarians who live there seem to have more energy than their brethren on the Danube. The Servians and Bulgarians dislike the Greeks, who in turn despise them: indeed the Greeks live on far better terms with the Wallachians or even the Albanians. On the other hand, the Wallachians and Servians dislike one another, and the former consider the Bulgarians and Servians as their vassals; because they say it was so during the Roman empire, and also because Servia contains no large towns like Wallachia, nor any nobility; nor is there the same wealth and luxury among the *Boyards*, many of whom in Wallachia are very well informed, and have given their sons a fashionable foreign education. It must, however, be remarked, that the Wallachian peasant is, like the Hungarian one, a mere serf; whilst the Servian peasant is a freeman with landed property. I may also add that, although the Servians have removed by various emigrations into Hungary, the Bannat, and Syrmia, and even beyond *Fünfkirchen*, they still appear to entertain a national antipathy towards the natives of these countries, although living in the same kingdom, and under the same laws.

The Turks subdued the Servians in 1389, and the Greeks in 1473: the former began their struggle for freedom in 1804, and the latter in 1801. The Greeks, from the commencement of their war, received foreign assistance of all kinds, and they now have a foreign king, and are pretty deeply in debt; whilst the Servians, who conducted their war almost entirely without assistance, and at their own expense, have a government of their own choosing, with very few of those foreigners, so numerous in Greece, who, in many instances, probably attend less to the interests of the country than to their own. It may be added, that after a state of war, or at least of uncertainty, for nearly twenty-five years, the Servian government has a good deal of money in its treasury, and the country is in a quiet state, and entirely free from robbers. These differences in the present condition of the two countries arise from various causes, as, for instance, the different character of the inhabitants; for the Servians are much less inclined to be turbulent, are less vain, and come less into contact with other nations. Besides, although the Servians have had severe wars to sustain at different parts, Greece, from being so much surrounded by the sea, would have been exposed to greater danger if her seamen had not been superior to the Turks. In Servia, also, there was fortunately only one single people, whilst the Greeks were mixed up with the Albanians and other tribes.

The Servian war of independence had its origin in the Servians being obliged to defend themselves against some rebels to the government of Constantinople: when once in arms, they remained so until they had gained their point, the Turkish government being forced by their bravery, as well as by foreign influence, to grant their demands.

The Servian is happy because he is free from Turkish exactions, and has little money to pay to his government; each married man being only obliged to contribute thirty francs annually. The whole amount of this tax, however, is not collected from each person; but the chief magistrates of the districts, who are well acquainted with the different circumstances of the people, tax them more or less according to their property, until the average sum of thirty francs is made up. All this, as well as the paying in of the money to government, is effected almost without any of those expenses which would be incurred in other countries. Servia is divided into seventeen Isprabnikats, and each Isprabnik or Burgomaster has two or four counsellors, a secretary with two or three assistants, and some young men who are commencing their studies in the civil department. They have a kind of criminal code, but no civil or commercial one; nor are there any laws applying to the forests; nor have they as yet made any arrangements for the registration of mortgages. There are no advocates in Servia. Testaments are either written out, or sometimes merely verbal directions are given. They have no large prisons. At Belgrade the merchants have formed a kind of corporation, which gives decisions in commercial cases. The number of *military* on service is extremely small; but each person is obliged to serve in his turn. The army has three generals, two of whom are brothers of Prince Milosch; and the third, General Danilovitsch, has charge of the troops at present. There are five *Polkovnike* or colonels; of whom three are commandants, or somewhat higher in rank. Under them are the superior captains of the district, and, under these, the captains of the parishes. Each colonel has from three to seven superior captains under him. There are barracks at Belgrade, and also at Kragojevatz, where there is a military hospital under the direction of medical men from Hungary and Servia. The whole Servian force, when called out, may amount, it is said, to 60,000 men; and it would be easy for the government to assemble some thousands at any spot in a few days. The police is strict: passports are demanded; and the different parishes are made liable for any robbery or other crime which may be committed within their bounds.

The *clergy* are under a metropolite, Peter Johanowitsch, Archbishop of Semendria, and formerly one of the secretaries or ministers of Prince Milosch. He is now quite independent of the metropolite of the Hungarian Servians at Carlowitz, and is only in a slight degree under the authority of the Patriarch of Constantinople. He is well paid by the government, and also possesses some private estates. Besides the Archbishop there are three Bishops, viz. at Timok, Schabatz, and Tschatschak; and there are still twenty monasteries, containing 100 monks.

The only gymnasium in Servia is at Kragojevatz. There are sixteen schools paid by government, besides which, some few parishes have their own schools. A printing-office has been established at Belgrade, as also a type-manufactory: they print very well, and have already put into circulation many useful books, the most of which are translations, together with some new works, poems, &c. The only Servian newspaper is printed here; but the circulation of this is still prohibited in Hungary. The best national poet seems to be Milutinowitsch, who is now at Leipzig, engaged in printing two new works, and in collecting historical information. He is a native of Bosnia, and has a pension from Prince Milosch. The same reward was also conferred on Vuk Stephanowitsch, now residing in Vienna, who was the first that published a collection of the old poetical Servian songs. Last year he published at Stuttgart a small account of Montenegro, and he also supplied Ranke, of Berlin, with materials for his History of the Servian Wars and Revolution.

Prince Milosch governs by means of a few secretaries and counsellors, some of whom are Hungarians. The name of his foreign secretary is Ivanovitsch. He has no court, and no magnificent houses, like those of European princes, but only good *Konaks*, or stone-houses of one story, like those of his people. He has no sentinels stationed before his door; but opposite the house, there is a small guard of ten soldiers for the protection of the treasury. He has two sons, Milan and Michael, of sixteen and twelve years of age, and two daughters, who are married in Hungary. He lives in a very quiet way, and generally resides at Kragojevatz; but he also has *Konaks* at Topschid, near Belgrade, at Poscharevatz, and perhaps in other places. From time to time he assembles together the chief men of the state. The Prince himself, and also his officers and the troops, are clothed in the complete European dress; but the captains still retain their beautiful red Turkish uniform. The government is of the least complicated and most economical kind, but at the same time perfectly effective. The money in use is Austrian as well as Turkish; and we even find the Austrian paper-money in the large towns.

The whole country is intersected by great roads, which are not at all macadamized, so that, although they are good in summer and during dry weather, they become extremely bad in the winter. There are a great many small bridges, and at Keupri there is a pretty long one. But the country is greatly in want of engineers, of medical men, and particularly of good artisans, such as carpenters, joiners, locksmiths, and cartwrights. Situations have been formed for 25 superior medical men and 64 surgeons; but some of them are still vacant owing to the want of proper men. New churches are being built everywhere; and towns and villages are rebuilt in more healthy situations. An ordinary new house for a peasant will cost him only 12 or 20 francs, besides his own labour, as he gets the wood from Government. A potash manufactory has been established; but glass, delftware, cloth, and beet-root sugar-manufactories are still awanting, and would probably turn out most advantageous speculations, as the trade with the rest of Turkey is free. It is curious, in the infancy of the arts, to observe the few instruments of the carpenter, &c. Planks are squared with the axe in most places; and there is only one saw-mill in the whole country: the use of the large double-handed saw is quite unknown. In these respects the Servians are very far behind the Bulgarians and Greeks; as these last have not only large tanneries, but also manufactories of morocco leather, articles of copper, cloth, cotton, cotton-stuffs, carpets, and shawls; as also fine establishments for dying thread as well as some kinds of cloth. I myself saw at Scharkoe nearly the whole population occupied in making beautiful carpets: the colours are bright, but sometimes rather deficient in regularity, as they are worked in by the hand without the aid of proper instruments or patterns. This work is chiefly done by girls, who sit on the ground after the Turkish fashion, the frame being placed vertically before them: their wages only amount to five francs per month. Could these carpets not come into competition with others in foreign markets?

The shawls are also worked by the hand. There are pretty large manufactories in Salonichi, Istip, Monastir, Sophia, and Eski Sagra; and all the necessary articles for horses are made very good throughout the whole of Turkey.

In *Agriculture* the Bulgarians seem to be farther advanced than the Servians; for the former have fine gardens, and cultivate more culinary plants than the latter, who are contented with maize, corn, kidney-beans, water-melons, and the common fruits. One chief branch of agriculture in Servia, and some parts of the country occupied by the Wallachians, is the cultivation of plum-trees for making brandy. For this reason the villages in Servia often seem to be surrounded with forests of plum-trees. In Bulgaria, and especially in Macedonia, these trees are not cultivated; because, the country being warmer, vines are more plentiful. In Servia the occurrence of vast forests of oak and wild pear trees has given rise to the rearing of pigs on an extensive scale. These animals are constantly in the woods; and the Servians export such numbers of them to Austria, that this trade forms their greatest revenue. The Austrian government could do them a great deal of harm by prohibiting it; but in that case the price of pigs would rise greatly in Hungary and Austria, where pork is much used by the common people. In the other parts of Turkey pigs are occasionally seen, but never in such numbers; nor indeed could they alway be reared on the same extensive scale, owing to the want of large forests.

The Bulgarians have a great talent for irrigation, and for this purpose they conduct the smallest streams of water from great distances: but they too often destroy the roads by using them as channels for the water. They cultivate maize, corn, the vine, cotton, plants for dying and for making oils, a little rice, &c. The vineyards afford a good fiery red wine like that of

Cahors. The cultivation of the olive-tree is only to the south of Salonichi, and is chiefly in the hands of Greeks.

They plough with oxen, which have their heads placed in two square pieces of wood. The hay is kept in the open air either in large piles or on low trees, on the branches of which wooden frames are sometimes placed. Considering the activity of the Bulgarians, it is quite certain that if they were under a wiser administration, all the ground which could be cultivated in Turkey would soon be covered with the most luxuriant crops. At present these are seldom met with; as they occur only in some of the great valleys; in other parts too many thistles are to be seen in the corn-fields. The hiring of men and women from the high lands for the labours of the harvest is a general custom; and in summer one meets large bands of young women and men travelling about. The Bulgarians, like the Servians, do not seem to understand the proper management of land by a rotation of crops; and for this reason fields sometimes remain uncultivated for years. Potatoes are unknown throughout the whole of Turkey with the exception of Montenegro, where they were introduced above twenty years since by the late Valadika Peter Petrovich. The art of improving the quality of fruit-trees seems also to be very little known in Turkey, and scarcely any thing is known about the proper application of manures to the soil. Lastly, throughout the whole of Turkey, no care whatever is taken of the forests; a circumstance which requires the serious attention of the Turkish government, particularly in regard to the mining districts, as, from want of wood, owing to the present careless way of cutting down the trees, some of the mines may soon become useless. There is much more wood in northern than in southern Turkey; but even there people should not be allowed to commit such havoc in the finest oak-forests. The largest trees are burnt down below by travellers or shepherds merely for the sake of a fire; and the trees afterwards fall upon the road or in the forest, or perhaps rest suspended on other trees, nobody paying any attention to them. Some trees remain half burnt, and occasionally parts of the forest catch fire from carelessness. No regulation is enforced as to the cutting of the trees. It is said that some regulations have been made in Servia to remedy these evils, but they are of little avail, as the custom has been so long continued. In Southern Turkey many places are already devoid of wood; and, where the country is hilly, the water carries away the vegetable earth, and there only remain bare rocks and dry beds of streams, as in so many parts of France, which, from like causes, present the same barren appearance. The Servians and Bulgarians are accustomed, like the Italians, to take a siesta during the hottest time of the day; but the climate almost renders it necessary, and they cannot therefore be called lazy: indeed, the soil is so rich that they do not require to work so hard as the Germans.

The *Commerce* of Turkey is chiefly in the hands of the Armenians, Jews, Greeks, and Bulgarians. All the various merchants, with the exception of the Servian, have correspondents in many of the commercial parts of Europe, and elsewhere; but the Servian is less inclined to commerce than to agriculture; and, if he does enter into trade, so soon as he has made a little money in foreign countries, he returns to Southern Hungary or Servia. The Armenians are worse than the Jews, and hate the Greeks and Servians; but the Turks reckon them most faithful servants. It is well known in what way the Armenian bankers in Constantinople dispose of the revenues of the state, and keep the Pashas in a sort of dependence, which is most injurious to the interests of the state and the happiness of the people. If Sultan Mahmud succeed in effecting a change in the financial part of his administration, he will have cut the gordian knot, and will easily get the better of other difficulties.

It is astonishing to see the high interest at which Armenians and Jews lend their money, by taking advantage of the misery of the people, and their inability to pay the exactions of the Turks. The very men who sometimes urge the Pashas to levy an extraordinary tax are those who derive most benefit from it; and, from receiving in pledge the crops and other things, they may with justice be called the *leeches* of Turkey. I conversed with one very rich Jew at Monastir, who told me that he lent money to the merchants at 25 per cent. per annum, and to the Albanians at 25 per cent. per month! He mentioned that he seldom lost money, and was never robbed on the high way; a fact confirmed to me by the bankers in Vienna who trade with Turkey. This person was living in a small house, the walls of which were so bad that they could have been easily knocked down by a hammer, and yet his money-chest was perfectly secure. The trade of Turkey seems to be always in favour of this rich country. The chief exports are cotton, some silk and a great quantity of wool, leather or skins, oil of various kinds, and leeches.* For the home market they manufacture woollen and cotton cloth, silk and cotton stuffs, carpets, shawls, and some coarse iron instruments, made at Egri Palanka, Vrana, and Somakov: at which latter place there is a manufactory of iron balls. They receive from foreign countries all their paper, earthenware and a small quantity of porcelain, glass, a great variety of iron articles even of the coarser kind, such as sickles, &c., a great quantity of dyed woollen, cotton and silk stuffs, watches, musical boxes, and various kinds of musical instruments. The Turkish trade seems to be chiefly enjoyed by the Austrians and English: French goods are scarcely seen at a distance from the maritime districts; but there the higher orders are for the most part supplied from France with articles for the toilette. I am surprised that no one has yet tried to export the best qualities of red wines, which seem admirably adapted to the English and northern taste. In the country a bottle of wine costs only two or three sous (French); and it has no taste of goat skins like the common Greek and Spanish wines. At Samos there is an excellent Muscat wine, which costs only five sous per bottle at Salonichi.

Although Turkey possesses no gold mines, yet mines of silver, lead, and iron are known and worked. Bosnia seems to be particularly rich in this respect, and I may give as examples Szerbonik, Maidan, Brunzoni and Bosna Serai. But, in order to derive proper advantage from these resources, well educated miners should be appointed to each mine; and some of the mines should be taken under the control of government, proper encouragement being given to companies to work others.

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* The trade in leeches is already extended to Asia Minor; and at Semlin, vehicles are always ready to convey them onwards. The leeches from Bosnia and Albania are sent to Trieste or Italy; and the marshes in which they are found are let at an annual rent.

For the discovery of new mines, engineers should be appointed to examine the principal chains of mountains in Turkey; and the mines should be worked by paid miners, in order to allay the fears of the Christians; who are afraid to give information about mines, in case the Turks should oblige them to work, and thus add a new burden to their oppressive load of taxes.

I may now add a few remarks on the moral and sanitary state of the population. It is very remarkable, that, throughout the whole of Turkey, the traveller meets with scarcely any beggars: indeed, almost the only ones we saw were a very few poor Turkish women. There certainly are poor, for misfortune and improvidence exist every where in a greater or less degree, but they are in much smaller numbers than in the rest of Europe; as living is very cheap, and it is very easy to get sufficient money for purchasing the necessaries of life: besides, for this reason, the poor can be more easily assisted by their relations or by other people. In general the men are too proud to beg on the roads; but, if forced by absolute necessity, they would rather become *Haiduks* or highway robbers, and ask money pistol in hand. At present there are no robbers excepting on the borders of the kingdom of Greece and of Montenegro; and even there a very few stations of *gendarmes* are sufficient to secure safe travelling for the merchants, indeed, if twelve or fifteen men are enough to protect twenty leagues of hilly country, one may safely say that the country is quite secure. The soldiers at the stations do not even always demand a *Backhish* or *pourboire;* but the payment of a few *paras* is not much for a traveller when it secures his safety. One circumstance which struck me forcibly was, that I nowhere saw people employed gleaning in the fields after the crops had been cut down. When one establishes his bivouac at a distance from a village, he occasionally commits the trifling theft, when the maize is nearly ripe, of taking a few ears of it to be roasted at the fire; great quantities of maize next the roads are also eat by the horses in travelling along, as the fields have no inclosures.

The whole population of Turkey exhibit a great deal of good sense, and the traveller is also surprised to find so few persons of coarse manners. Every thing is done without noise and speeches; and the masters of the post-houses, too often unpolite in other countries, are here quite ready to attend to their duty. The Greeks on the contrary talk a great deal more, and sometimes seem to be quarrelsome.

The population is very healthy for various reasons: their mode of life is simple, and all weakly children die early for want of medical advice· there are scarcely any preserved in life by those artificial means which tend so much to increase the population in Europe, especially in large towns; and besides, healthy children are much more likely to be the offspring of healthy than of sickly parents. For the same reason we observe so few insane people and persons who squint; as both these affections arise from weakness in the nervous system. The chief causes of insanity, love and religious fanaticism, have little influence in Turkey, owing to the mode of life. For like reasons, and owing to the absence of many of the factitious wants of Europe, cases of suicide and duels are hardly known: indeed it appears, that the Servians who emigrate from their own country into Hungary, wisely retain their dislike to those two evils of modern society. Suicide, however, excites no very great sensation, as life is held cheap; as for duels, the Turks think it no disgrace to avow their love of life, and refuse this mode of settling between right and wrong; although our wisest legislators are still obliged in a certain degree to allow the practice in the present corrupt state of society. It is the general custom to marry pretty young. The *goitre* is a rare disease, and there are few blind people, or when cases of blindness occur, they generally arise from accidental causes, ill treated diseases, or the frequent ravages of the smallpox. Vaccination is employed in Servia and some of the large towns of Turkey. There are few maimed or bandy-legged people.

The number of cutaneous diseases seems to be small, and much less than in Asia Minor. With the exception of the Jews, the other inhabitants are, for their state of civilization, pretty cleanly; but lice are more common than in the middle of Europe : the fashion of shaving nearly the whole head is a good one in this respect. The chief diseases are intermittent and typhoid fevers, nervous affections, and inflammatory diseases to these the plague is to be added in the maritime towns.

As the Turks have more leisure and more factitious wants than their Christian fellow-subjects, they are liable to lowness of spirits and hypochondriacal and other nervous affections, from which the Christians are quite exempted. On the other hand, the stoicism and good temper of the well-bred Turk, and his love for children, and all kinds of animals, are well known. He also possesses the noble quality of faithfully keeping hi promise, and to such an extent is this relied on, that the most important transactions are settled by merely shaking hands with each other; a practice which has also been partly adopted by the Christians in smaller matters. I may add that the general ignorance of writing has induced the Turks to make use of a particular apparatus for calculating sums of money, &c. By means of small pieces of wood cut into various shapes, they transact their business as well as our best writers. In many of the inns, instead of a writing table, these pieces of wood are to be seen hanging up at the corner of the innkeeper's room.

In regard to the social life, in the interior of the country, at a distance from the maritime towns, in which one finds, in a greater or less degree, European fashions and entertainments, each family is obliged to seek its chief pleasure in its own circle; for entertainments are seldom given, or only at certain fixed times, or at marriages, &c. The men meet every day in coffee-houses and public places, and take a walk together : the ladies remain at home, visit each other, or occasionally walk together. For these reasons an unmarried European finds himself too isolated, and has great difficulty in reconciling himself to the different mode of life. *

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* In a late communication from Dr Boué, there is an interesting sketch of the Geology of Central European Turkey, which will appear in our next Number.—Edit.

On the Cause of the Temperature of Hot and Thermal Springs; and on the bearings of this subject, as connected with the general question regarding the Internal Temperature of the Earth. By Professor Gustav Bischof of Bonn. Communicated by the Author. (Concluded from p. 164.)

Chap. XVIII.—*In what proportion to the height above the lowest points of the Earth's surface is the decrease of the Temperature of the Air and of the Soil? and is this Decrease the same at all parts of the Earth?*

There are various causes which influence the decrease of temperature with the height. Such are,

1st, The configuration of the earth's surface.
2dly, Local circumstances and climate.
3dly, The seasons.
4thly, The time of day.

Let us examine these causes more closely.

De Saussure* already remarked the more rapid decrease of the temperature on steep mountains, and Von Humboldt was led to the same conclusion, by comparing the temperature of the air on elevated plateaux, with that of the summits of mountains. He found, namely, that such plateaux, or narrow valleys, were warmer than the steep declivities of mountains, or than such strata of air which, instead of being in immediate contact with projecting parts of the earth's surface, rested upon other strata of air. On the other hand, the decrease of temperature from the level of the sea will be slower on extensive and gently rising chains of mountains, than on mountains having a steep ascent. As the lofty *American* chains of the *Peruvian* and *Mexican Andes*, to the height of *St Gothard*, form one immense mass, and up to that height comprise a surface of 120 square geographical miles, the decrease of temperature in the inhabited regions of the *Andes* is slow, but becomes suddenly more rapid where the less massive rocks rise 10,800 to 12,000 feet in isolated cones upon their broad basis. Thus Von Humboldt found a decrease of temperature of 2°.25 in 996 feet, between the level of the sea and the *Indian* village of *Calpi;*

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* Voyages dans les Alpes, iv. § 935.

but the same decrease took place in every 618 feet, from *Calpi* to the highest point on the chain of the *Chimborazo*, or in the last 8292 feet. But taking the whole column of air of 18,072 feet together, the decrease of 2°.25 took place in every 714 feet. Between the surface of the sea and *Quito*, *Popayan S. Fe de Bogota*, and *Mexico*, which lie on that extensive plateau, we find from their respective mean temperatures a difference of elevation of 964 to 1090 feet* for every degree of temperature. Ideler† shews, that the influence of plateaux and broad ridges on the decrease of temperature in the *Himalaya* mountains is still more considerable.

Besides these general effects of the configuration of the earth's surface which are independent of latitude, there are others which are closely connected with the geographical situation of the place. Thus, under the equator and in low latitudes, the generation of heat must be greater on plains than on steep mountains, because the rays of the sun fall almost or quite perpendicularly upon the former, while with the latter they make an acute angle. In higher latitudes the contrary takes place on the sides of mountains which lie opposite to the sun. In the northern temperate zone, there may, therefore, be a slower decrease of temperature on the southern declivity of a mountain than on the northern or on gently sloping hills.

Climate and local circumstances, which have the effect of lowering the limits of perpetual snow, must, of course, also have some influence on the decrease of temperature. For the nearer the limits of perennial snow are brought at any place to the surface, the more rapid must be the decrease of temperature in proportion to the height. In the middle of an extensive chain of mountains, the decrease of temperature must consequently be more rapid than on both sides where the warmed air of the plains still operates.‡

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* Von Humboldt, in Gilbert's Annal. xxiv., 26-36 ; and xxxi, 369.

† Ideler ueber die Abnahme des Wärmestoffs mit der Höhe, p. 103.

‡ According to De Saussure (Voyages dans les Alpes, § 942), the limits of perennial snow are lowered more than 600 feet, by accumulations of snow, or by water resulting from the melting of snow. In like manner, Ramond asserts that the limits of perpetual snow, under the latitude of the Pyrenees, form a curve which descends from both ends of the chain to the middle, and which is therefore its lowest point. V. Buch, in Gilb. Ann., xli. p. 18.

In the same manner as glaciers and large accumulations of snow are enabled to lower the limits of perennial snow, so will they also have an influence on the decrease of temperature. From low valleys up to the glaciers, the decrease of temperature must, therefore, be more rapid; and from thence to the highest points of the snow-capped mountains it must advance more slowly. What holds good with respect to the glacier, also obtains for narrow fissure-like valleys in which perennial snow is also not unfrequently found.*

The decrease of temperature with the height cannot be the same at all seasons of the year, nor at all hours of the day, as the daily and yearly variations of temperature decrease in proportion to the height, and, indeed, at a certain height entirely disappear. Between this height and any point of the earth's surface, the temperature must consequently decrease more rapidly at the time of the maximum yearly or daily temperatures, and more slowly at the time of their minimum. So that the greater the yearly and daily differences of temperature are, at a certain place, the greater will the variations in the decrease of temperature also be. But since, in general, the former are greater the further we depart from the equator, the latter must increase proportionately.

Between the tropics, where the thermometer only oscillates a few degrees, as well on the sea-coast as on the elevated plains of the *Andes*, during the whole year, the yearly and daily variations in the decrease of temperature must be almost equal to nothing. Thus, the greatest yearly difference of temperature in the air at *Cumana*, 12 feet above the level of the sea, is stated, by Don Faustin Rubio,† to be only 4°.19 ; and at *Quito*, 8969 feet above the sea, it is, according to Hall and Salaza,‡ between 4°.14 and 4°.50. Now if, for example, a constant temperature should be found to prevail, at a height of 18,072 feet on the *Chimborazo*, where Von Humboldt made his observations; whilst the yearly variations of temperature on the level of the sea amounted to 4°.19, this would only give a yearly difference of 58 feet in the adopted decrease of 2°.25 in every

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* See Wahlenberg de Veget. et Clim. in Helv. Sept., p. 68.

† Annal. de Chim. et de Phys., t. xxii., p. 303. Von Humboldt's Reisen, part ii., vol. iii., p. 133, in note.

‡ Annal., t. liii., p. 232.

776 feet. But this difference would be smaller, if the temperature should suffer oscillations also at the higher point of observation.

The nearer the points of observation are to the sea, the less material is it at what hour of the day the observations are made; for the daily variations of temperature become smaller as we approach the sea-coast, and are the least considerable on the open sea, especially between the tropics. Thus Humboldt observed that the greatest daily differences of temperature, during his voyage from *Europe* to *Cumana*, seldom exceeded 2°.70 to 3°.60.[*] The same has been observed by Peron,[†] Horner and Langsdorf,[‡] Lamarche,[§] and other travellers.[||] The small islands on the ocean have no influence upon this phenomenon.

The effect of climate is, however, felt on land, even in the torrid zone. Thus, differences of 8°.55 to 14°.85 were observed between the daily mean temperatures of *Cumana* and *Caraccas*, from the 29th November 1799 to the 19th January 1800.[¶] Boussingault[**] found a difference of temperature of 5°.94 between eight o'clock in the morning and noon, on the 9th August 1830, at *Zupia*, 3770 feet above the sea. Von Humboldt[††] found the nights of *Cumanacoa* 3°.60 to 5°.40, and the days 7°.20 to 9°.0 cooler than in the port of *Cumana;* and yet the difference of elevation between these two places is only 1548 feet. On the plain of *Cocollar*, 2448 feet above the sea, the thermometer sunk at night to 57°.20, after having been at 72°.50 in the day-time; the temperature in the night was, therefore, 15°.75 lower than on the sea-coast.[‡‡]

In the temperate zones, where the yearly and daily variations of temperature are so considerable, the decrease of temperature with the height must shew very considerable variations also. The corresponding observations at *Geneva*, and on

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[*] Voyage, part ii., p. 74.

[†] Peron, Voyage, t. i., p. 32.

[‡] Krusenstern's Reise, vol. iii. Appendix.

[§] Annal. de Chim. et de Phys. vol. v.

[||] See Kämtz, Lehrbuch der Meteorologie, vol. ii., p. 17.

[¶] Von Humboldt's Reise, part ii., p. 491.

[**] Annal. de Chim. et de Phys., vol. liii., p. 229.

[††] Reise, part ii., p. 48.

[‡‡] Ibid. p. 75.

St Bernhard,[*] give a decrease of temperature of 2°.25 in 546 feet in July, and in 946 feet in December; making a difference of 400 feet between the coldest and the warmest month.[†]

Equally large differences are found between different hours of the day. Thus, the following results were obtained from corresponding observations at the following places:—

	Difference of height corresponding to 2°.25 difference of Temperature.	Hour of Observation.
Col du Géant, July,[‡] . .	538 feet.	2 P. M.
	808 —	4 A. M.
Zürich and Rigi Culm, June,[§] .	453 —	5 P. M.
	700 —	5 A. M.
Geneva, Zürich, and Faulhorn, from 11th September to 5th October,[§]	594 —	4 P. M.
	1174 —	5 A. M.
In heights of 1000 to 2400 feet between *Poppelsdorf*, the *Siebengebirge*, and the *Hohenacht*, I found, on the 28th March and the 1st June 1833, by corresponding observations, made every quarter of an hour of the day and night, . . .	1148 — 667 —	in the Day.
	1246 — 917 —	in the Night.
At the time of the daily maximum of temperature, . . .	264 —	
At the time of the minimum, .	1560 —	

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[*] Bibliotheque Universelle, t. x., p. 173; t. xiv., p. 19.

[†] In the spring, on the other hand, when the snow is already melted in the low valleys of the Alps, but still covers the summits of the mountains, the difference of temperature between the plains and the mountains is so great, that a difference of 2°.25 may sometimes be reckoned upon, in a difference of height of 60 to 160 feet. During the five days which Von Humboldt and Gay-Lussac spent, in the month of May, at the convent of *Mont Cenis*, they found the thermometer constantly 27°.00 to 33°.75 lower than at *Lanslebourg*, although the difference of height between these two places is hardly 1944 feet. Gilbert's Annal., xxiv. p. 22. Hugi found the temperature on the *Weissenstein* higher than at *Solothurn*, in the winter of 1829–30. Kastner's Archiv für Chem. u. Meteorol., iv. p. 130. On the 6th January 1833, I found a mean temperature, from observations made every half hour from sunrise to sunset, at *Poppelsdorf*, + 23°.49, and on the *Petersberg*, 838 feet higher, only + 23°.68. On the other hand, on the 29th December 1832, I found, from similar corresponding observations at *Poppelsdorf* and *Burgbrohl*, which only lies about 250 feet higher than the former place, but six or seven leagues to the south-east of it, a mean of + 45°.36 for the former, and + 33°.37 for the latter place. It was just on this day that a frost set in with a south-east wind, and thus, the cold air descending into the deep valley of *Burgbrohl*, suddenly produced this great decrease of temperature between the two places.

[‡] De Saussure, Voyages dans les Alpes, § 2050.

[§] Kämtz in Poggendorff's Ann. xxvii., 345.

At what height do the yearly and daily variations of temperature cease? During the above observations, the extent of the variations of temperature were,

In Zürich, . . .	12°.73
And at the same time on Rigi Culm,	6°·90
Again at Zürich, . .	21°.58
And at the same time at Geneva,	14°·92
And on the Faulhorn, . .	8°.50

From this it indeed appears that the extent of the variations of temperature is already considerably diminished in a difference of height of 4203 feet, between *Zürich* and *Rigi Culm*, and in 6866 feet, between *Zürich* and the *Faulhorn;* but we should be drawing our conclusions too hastily were we to infer that the point where the variations of temperature entirely disappear cannot lie very high. The extent of these differences of temperature does not only depend on the height, but also on other causes, as is proved by the great difference between *Zürich* and *Geneva*, which places are both at the same height above the level of the sea. Even on the summits of the highest mountains in *Switzerland* variations of temperature are still found. Thus, it sometimes rains on the top of *Mont Blanc* during the hot summer months, and the snow sometimes begins to melt there. Zumstein[*] observed a temperature of + 51°.12, on the 12th August 1819, on *Mont Rosa*, 13,920 feet above the sea. Parrot[†] experienced an oppressive heat on the Lesa glacier, on the southern side of the Rosa, at an elevation of 10,576 feet above the sea. Between the tropics, the point where the variations of temperature cease, is probably not situated so high as in the temperate zones; because, on the level of the sea, the yearly and daily differences of temperature are there confined within very narrow limits. However, Boussingault[‡] relates that, on the 26th December 1831, a thermometer, suspended free in the shade of a rock on *Chimborazo*, at a height of 18,480 feet above the sea, and 3654 feet above the limits of perennial snow, the greatest height to which mountains have yet been ascended by men, and where the mean temperature of the air is probably + 23°.0, shewed + 46°.04; and that the snow was in such a state as to be about to melt. So far, then, it is certain that, at the highest points of our globe, there are still variations in the temperature of the air. Whether this disappears at the height of 34,158 feet, as Von Lindenau* supposes, must remain a question.

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[*] Gilbert's Annal., vol. 63. p. 391.

[†] Schweigger's Journ. xix., 386.

[‡] Poggendorff's Ann. xxxiv., p. 206, and Edinburgh New Philosophical Journal, vol. 19. p. 98.

But the variations in the temperature of the soil disappear in accessible heights, even at a small depth beneath the surface. According to Boussingault's observations (Chap. VIII), in the *Cordilleras*, the temperature of the soil is there constant even at a depth of one foot, on the level of the sea. As in higher latitudes the depth to which the influence of the external temperature penetrates, is much greater than between the tropics (Chap. VIII.), the same must also be the case at all heights accessible to man.

According to the observations which I have been making for a year past, in front of the chemical laboratory and on the top of the *Löwenburg*, situated 1173 feet higher, the yearly variations of the temperature of the earth at the depth of four feet, amount, at the former place, to 19°.12, and at the latter to 15°.63. For reasons which will be given below, I think, that the yearly variations of temperature on the *Faulhorn*, 8178 feet above the sea, and 6784 feet above the *Löwenburg*, are not less considerable at a depth of four feet. It is, however, to be expected, that at the higher points of the Alps, where the earth is covered with perpetual snow, the temperature of the soil at this depth below the surface is constant.

Can the decrease of temperature with the height be considered as an arithmetical progression in the strictest sense of the word?

Von Humboldt's observations, according to which, the difference of height corresponding to 2°.25 difference of temperature is greater, the greater the difference of height between the two points of observation, seem indeed to indicate, that between the tropics the decrease of temperature becomes less and less rapid from the level of the sea nearly to the regions of perpetual snow. And Humboldt† himself doubts whether this decrease does follow an arithmetical progression. Schmidt[*] has arrived at the same conclusion from considerations of a different nature. Observations at eighteen points in *South America*, made exactly where Boussingault made the observations communicated below, seem also to indicate a diminishing decrease of temperature.[†] But it is probable that this apparently diminishing decrease of temperature is due to another cause. For, since observations by day give a more rapid, and by night a slower decrease of temperature in the air, and consequently the mean between the two is the expression of the real decrease, we shall obtain a more accurate result the greater the difference of height between the points of observation is, where *single* observations only are made, from which the decrease of temperature is to be calculated; for, the daily variations of temperature at the lower point will have the less influence on the difference of height in which the temperature decreases 2°.25, the greater the difference of height between the two points is. So that, if observations be made at different heights at the time of the daily maximum temperature, an apparently retarded decrease of temperature will be found; and, if at the time of the minimum, it will be apparently accelerated. But by far the greater number of observations at different heights have certainly been made nearer to the daily maximum than to the daily minimum of temperature. We are, therefore, not justified by these observations, in concluding on a diminishing decrease of temperature from the surface of the sea to the greatest heights accessible to man, namely to the limits of perennial snow. Gay-Lussac's observations during his aeronautical excursion are equally insufficient to justify a contrary conclusion.[§]

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* Monatliche Correspondenz, xxi., 119. See Ideler, p. 103.

† Gilbert's Annal. xxiv. p. 48.

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[*] Gilbert's Annal. vol. lxii. p. 309.

[†] Ibid. vol. lxii. p. 309.

[‡] Mémoires d'Arcueil, iii. p. 580. Semanario del Nuevo regno de Granada. vol. i. p. 273. ii. p. 93–341. See Ideler, p. 95.

[§] Gilbert's Annal., xxiv., p. 28. and xxxi. p. 32. Gay-Lussac himself remarks, that his thermometers did not keep pace with the temperature. And how could the thermometers be expected to keep pace with the temperature of the air, considering the velocity with which an aeronaut makes his ascent? See also the remarks of Humboldt in the same, p. 34. and of Gilbert, p. 32.

The means of determining the decrease of temperature with the height, are:

1st, Observations on the temperature of the air.

2d, Observations on the temperature of springs; and,

3d, Direct observations on the temperature of the soil.

The next question to be asked is, Whether the decrease of temperature in the air, and that of the soil, in proportion to the height, follow the same progression?

If, in general, according to Chap. VI, there is no reason for supposing a difference to exist between the mean temperature of the air and that of the soil, this question is already answered. In this case the determination of the mean temperature of the air or of the soil at various heights, would be sufficient to solve the problem. That between the tropics there is no difference between the mean temperature of the air and that of the soil, we have seen in Chap. VI. In the temperate zone so few observations have been made, that the means of comparison are wanting.

From all that has been said it follows, that between the tropics, the decrease of temperature in the air may be found with a tolerable degree of exactness, by single corresponding observations, made at different heights. The most accurate results, however, are only to be got from the yearly mean. Von Humboldt* takes the mean of all his observations between the tropics at 654 feet difference of height for 2°.25 difference of temperature, for the stratum of air comprised between the surface of the sea and 3000 feet above it. As this philosopher made most of his observations in the day-time, it may be assumed, as we have said above, that they have yielded a somewhat too rapid decrease of temperature.

Let us compare this result with Boussingault's observations on the decrease of the temperature of the soil in the *Cordilleras*, between 11° north and 5° south latitude. This philosopher ascertained the temperature of the soil at no less than 128 places, at all heights between the surface of the sea and the limits of perennial snow, after the manner already described (Chap. VI).

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* Reisen, ii. p. 492. Formerly Von Humboldt supposed it to be 726.6 ft. Gilbert's Annal. xxiv. 48.

In order from these to calculate the decrease of temperature with the height, it was necessary to assume a fixed temperature at a certain height; a supposition which, indeed, strictly speaking, does not actually exist. We might have taken for our fixed point, either the temperature at the level of the sea, or on the limits of perennial snow. To take the former as a *terminus a quo* did not seem convenient, as the mean temperature of the places on the level of the sea varies between 78°.8 and 83°30, which difference would give a difference in the decrease of temperature, from the surface of the sea to the limits of perennial snow, of 71 feet in 2°.25. I, therefore, took 34°.92, given by Boussingault as the mean temperature on the limits of perpetual snow (14474 feet above the sea), as a constant value, and from it and the mean temperatures of the places below it, I calculated the difference of height corresponding to 2°.25 difference of temperature. This gave the 3d column of numbers contained in the following table. The heights given by Boussingault in metres are reduced to Paris feet, and the centesimal degrees to those of Fahrenheit.

NAMES OF PLACES.	Height above the Sea.	Mean Temperature.	Difference of Height corresponding to 2°.25 Difference of Temp.	NAMES OF PLACES.	Height above the Sea.	Mean Temperature.	Difference of Height corresponding to 2°.25 Difference of Temp.
Cumana, .	0	81°.50	713	Cayma, .	1,210	81°.72	650
La Guayra, .	0	80.60	727	Estanque, .	1,311	80.15	668
Rio Hacha, .	0	82.62	696	San Martin, .	1,330	79.92	672
Santa Marta,	0	83.50	686	Maracay, .	1,351	77.9	702
Baranquilla, .	0	82.17	702	Haguita, Choco,	1,431	79.02	680
Carthagena, .	0	81.50	713	Valencia, .	1,502	78.8	680
Panama, .	0	81.05	721	Neiva, .	1,597	77.0	704
Tumaco, .	0	79.02	754	Barquisimeto,	1,607	78.12	686
Guayaquil, .	0	78.8	757	Mariquita, .	1,687	77.67	687
Payta, . .	0	80.82	724	La Vittoria, .	1,727	78.12	680
Esmeraldas, .	0	79.47	745	Alto de Mombû.	1,794	77.0	693
Barranca Nuevo,	0	81.72	710	Antioquia, .	1,936	77.22	677
San Carlos, .	520	81.50	687	Tocuyo, .	1,936	75.87	704
Novita, Choco,	554	79.02	725	Truxillo, .	2,262	77.0	668
Honda, .	640	81.72	677	Tampo de la Orqueta, .	2,262	77.0	668
Giramena, .	665	81.05	689				
Tomependa, .	1,200	78.35	701	San Geronimo,	2,318	74.97	695

Names of Places.	Height above the Sea.	Mean Temperature.	Difference of Height corresponding to 2°.25 Difference of Temp.	Names of Places.	Height above the Sea.	Mean Temperature.	Difference of Height corresponding to 2°.25 Difference of Temp.
Truxillo, .	2,561	74°.97	682	Pamplona, .	7,113	61°.70	643
Villetta, .	2,582	77.9	638	La Baxa, .	7,243	59.0	703
Chami, à la rivière,	2,773	72.50	718	Alausi, .	7,480	59.0	680
Caraccas, .	2,881	71.37	732	Ubita, Soccorro,	7,495	61.7	611
Roldanillo, .	2,949	75.87	648	La Chorrera, .	7,673	61.02	612
Cartago, .	3,013	76.10	642	Sonson, Antioquia,	7,803	57.20	703
Ruga, . .	3,032	75.87	644	Pelileo, .	7,821	59.67	629
Toro, . .	3,044	75.87	643	Ubate, .	7,886	59.90	619
El Bordo, .	3,112	74.75	657	Santa Rosa de Osos,	7,938	57.65	676
La Moyarra,	3,133	76.32	633	Chiquinquira,	7,994	60.80	589
Guaduas, .	3,146	74.75	655	Pasto, . .	8,034	58.55	643
La Plata, .	3,226	74.75	653	Cuenca, .	8,101	60.12	595
Anserma Nuevo,	3,232	74.75	653	Paisbamba, .	8,107	55.40	731
Santa-Ana, .	3,257	71.60	706	Santa Fé de Bogota,	8,129	58.10	644
Llano grande,	3,340	73.62	665	Puracé, . .	8,160	55.62	719
El Palo, .	3,420	73.85	656	Ambato, .	8,246	61.02	563
Carache, .	3,623	70.25	708	Muechisa, .	8,314	56.30	679
Juanambû, .	3,629	71.15	693	Guaranda, .	8,378	59.0	597
Vega de Zupia,	3,771	70.70	691	Santa Rosa, .	8,446	57.87	618
Sombrerillo, .	3,912	69.12	715	San Pablo, .	8,532	57.20	629
Ibagué, .	4,072	71.15	662	Tunja, . .	8,649	56.75	633
Hacienda del Rodeo,	4,128	68.0	723	Caxamarca, Peru,	8,803	60.8	518
Arma, .	4,365	68.9	688	Latacunga, .	8,806	59.9	537
Marmato, .	4,389	68.67	690	Riobamba, .	8,834	61.47	502
Mandiva, .	4,392	74.52	590	Quito, . .	8,982	59.45	532
La Canada, .	4,669	70.25	644	Tusa, . .	9,059	51.8	670
La Orqueta, .	4,679	66.65	717	Chita, . .	9,142	53.60	677
Medellin, .	4,762	68.90	662	ElGuavo, Riobamba,	9,145	51.8	749
Merida, .	4,983	71.60	600	Mucuchies, village,	9,206	57.87	544
Cabullo, Venta,	5,039	66.87	683	Pucarà, near Ibarra,	9,219	58.55	530
Rodeo, Zupia,	5,260	66.65	676	Tulcan, village,	9,292	55.17	608
Estrella, Antioquia,	5,297	65.75	689	Malvasas, Popayan,	9,357	54.27	627
Quinchia, .	5,467	65.75	676	Tuqueres, village,	9,563	50.	775
Anserma Viejo,	5,503	63.50	729	Pinantura, farm,	9,711	52.02	666
Venta de Berruecos,	5,507	63.95	715	Callo farm, .	9,726	54.02	595
Popayan, .	5,568	63.50	724	Paramo de Hervé,	9,748	50.45	726
Riosucio, Zupia,	5,596	66.65	648	Vetas, village,	9,905	49.10	770
Ferme de Orthega,	5,651	66.42	651	Cumbal, village,	9,908	51.35	668
Bannos, .	5,876	62.15	736	Alto de Salaza,	10,545	47.30	766
Azufral du Quindiû,	6,085	61.02	749	Los Paramos,	10,773	47.30	725
El Cedrito, Hervé.	6,159	67.10	601	Lisco farm, .	10,923	47.97	664
Suatá, .	6,214	68.0	582	Pantano Vargas,	11,302	46.85	653
El Moral, Quindiû,	6,258	64.40	649	Metairie d'Antisana,	12,533	39.87	1,000
Loxa, Peru, .	6,433	64.40	636	Azufral de Juan,	12,678	38.97	1,151
Samenga, .	6,467	63.50	653	Snow limit, .	14,774	34.92	687 758
Aguadas, Antioquia,	6,765	61.70	672				
Zotara, Popayan,	6,944	60.8	679	Glacier of Antisana,	16,805	28.85	769
Almaguer, .	6,956	62.60	635				
Ibarra, . .	7,006	63.05	622	Mean,	. .	. .	679

If we look at the greatest differences in the third column of figures between places situated at about the same height above the sea, we find that they exceed only three times the above-mentioned difference of 71 feet. This justifies the conjecture that these differences are only due to the local influence of climate. At least on the sea coast no other cause can be imagined. The numbers of the third column shew neither an increase nor decrease up to the *Métairie d'Antisana.* It is true the highest number, 775 feet is found near the end, but the next, which is 757 feet, is found quite at the beginning of the series. The case is nearly the same with respect to the minimum, 518 feet. The two last numbers but one, 1000 and 1151 feet, are, however, astonishingly high, and from this it would appear that the decrease of temperature becomes slower near the limits of perpetual snow. But the last number, 769, is again about equal to the mean. The two last numbers but one, 687 and 758, were found from the differences between the highest and lowest mean temperatures on the level of the sea, and that of the limits of perennial snow. They shew the above-mentioned difference of 71 feet, which may be considered to a certain extent as the maximum effect of climate.

The means of the four columns of results in the table being so nearly equal, proves that between the tropics, from the surface of the sea to the limits of perpetual snow, a *constant* decrease of temperature takes place. However, *Métairie d'Antisana* and *Azufral de Juan* must be excepted.

The mean of all these results gives a difference of height of 677 feet for 2°.25 difference of temperature, between the surface of the sea and the limits of perennial snow.

This value only surpasses the mean, which Von Humboldt found from his observations on the decrease of temperature in the air, by 23 feet. It might, indeed, be conjectured that this excess was due to Humboldt's observations having been made in the day, and, therefore, gave a somewhat accelerated decrease of temperature; but, in general, no great weight can be attached to this difference, as the numerical elements which influence observations of this sort are very various.* Their corresponding so nearly seems rather to lead to the conclusion, that between the tropics the decrease of temperature in the air and in the soil follow the same law of progression.

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* The differences of height found from Boussingault's observations, for 2°.25 difference of temperature, for example, would have been smaller, if the temperature at the limits of perennial snow should be lower than + 34°.92.

For the reasons above given, the decrease of temperature in the air in the temperate zones can only be deduced from the known mean temperatures at different heights. Here, then, we are confined to the habitable regions. All that we as yet know of the decrease of the temperature of the air in the temperate zones is from the corresponding observations made during several years at *Geneva* and on the *St Bernhard.* The mean of these observations gives a difference of 777.5 feet for every degree of Reaumur, or 2°25 degrees of Fahrenheit. This result exceeds the mean of Humboldt's observations by 123 feet, and that taken from Boussingault's by 100 feet; but it is far lower than the former of these philosophers found for mountains having a very gradual ascent. But as *St Bernhard* is 24 leagues distant in a straight line from *Geneva*, and the difference of height between the two places is only 6477 feet, it may be considered as being a case of gradual ascent; and the temperature must decrease less rapidly than if the mountain had a steep precipitous ascent. To conclude that the decrease of temperature in the temperate zones is less rapid than between the tropics, would, therefore, certainly be premature.

The first question to be decided is, whether, in the temperate zones, as between the tropics, the mean temperature of the soil and of the air are identical, in elevated situations, as well as on the level of the sea. If this be found to be the case, we may, instead of the tedious observations in the air, which at uninhabitable heights are, indeed, quite impracticable, substitute observations in the soil, which, although also combined with difficulties, are yet possible to be accomplished. But this question can be most easily answered by the observations at the *Monastery of St Bernhard*, where the mean temperature of the air is known, and where meteorological observations continue to be made.

The simplest method of obtaining the temperature of the soil, as I have found, is, to dig a hole about four feet deep, and to sink a wooden case of the same depth into it, and then to fill up the surrounding space with earth or sand. The case should be just so large as to admit a common wine bottle. The bottle, filled with water, stands on a piece of board, which is half an inch smaller than the opening of the box. Over the bottle is placed another piece of wood, which is held fast to the former by iron wires, and to the upper board is attached a wire, which is long enough for the observer to be able to reach it with his hand. The space above the bottle is filled with a bad conductor, for instance with tow, and the box is covered over with a board, upon which some stones are placed to prevent the entrance of the rain water, and the whole is covered with moss to preserve it from unauthorized observers. After the lapse of four weeks the first observation may be made; for in that time the disturbances, caused in the thermometrical relations by making the hole, will have entirely subsided. However, it cannot be otherwise than advantageous to allow a longer time for the restoration of the equilibrium before the first observation. As at the depth of four feet the temperature continues to rise uninterruptedly from the minimum to the maximum, and to fall from the latter to the former, monthly observations are quite sufficient to give the yearly mean temperature. Indeed, there can be no doubt that 12 observations made in this manner will give a more exact mean than observations made three times a-day in the air, without being made also during the night.

I have found that, even where there is a difference of temperature of 22°.50 between the atmosphere and the water in the bottle, the temperature of the air has no influence on that of the water, during the first two minutes. But two minutes are quite sufficient for a sensitive thermometer to assume the temperature of the water.*

In this manner I have observed the temperature of the soil, since the 28th February 1835, in front of the chemical laboratory near *Bonn* and on the *Löwenburg*, which rises with a steep ascent from the *Rhine* to a height of 1173 feet above the other point of observation. The results of the first year of my observations are as follows:—

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* In the spring of 1835, I requested the monks of the *Monastery of St Bernhard* to add similar observations on the temperature of the soil to their valuable meteorological observations; but I have not yet heard whether my request has been attended to.

	Bonn.	Lowenburg.	Difference.
1835. February 28.	42°.12	39°.76	2°.36
April 9.	44 .60	41 .0	3 .60
May 10.	48 .87	42 .8	6 .07
June 14.	55 .40	47 .97	7 .43
July 12.	56 .52	50 .0	6 .52
August 8.	59 .22	52 .92	6 .30
August 30.*	59 .0	54 .05	4 .95
October 21	53 .60	50 .22	3 .38
November 9.	49 .55	47 .30	2.25
December 21.	43 .25	42 .35	0 .90
1836. January 13.	40 .10	40 .10	0 .0
February 14.	41 .0	38 .41	2 .59
Mean,	49 .44	45 .57	3.87

We see that the maxima and minima do not happen at the same time; but that they take place later on the more elevated spot, and that as the decrease of temperature varies according to the seasons, so does also the decrease of temperature in the soil vary. The mean difference of temperature between *Bonn* and the *Löwenburg* gives a mean decrease of temperature of 2°.25 in every 683 feet between the two places. This number is so nearly the same as the mean of Boussingault's observations, that we may certainly be allowed to consider the decrease of temperature with the height, between the tropics and in 51° N. Lat., as identical, at least to the height of 1173 feet.

In these observations another circumstance must be taken into consideration, namely, whether each two points of observation, at different heights, must lie in the same position with respect to the sun; that is to say, whether they must both lie in the shade, or be exposed to the sun; or has this no influence on the yearly mean? The point of observation in front of my laboratory lies during the whole year in the shade; that on the *Löwenburg* is in a little hollow on the top of the mountain, and, although it is sheltered from the direct action of the sun's rays by the surrounding bushes, is yet very much more exposed to its influence than the former spot. The place where I am making the observation, mentioned in Chap. VIII., for ascertaining the depth to which the influence of the external temperature is felt, is exposed to the sun during almost the whole year; if, then, the position of a place with respect to the sun, has any influence on the yearly mean, it would shew itself most distinctly between this spot and that in front of the laboratory.

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* I was prevented from observing from the 8th August till the 21st October by my journey to the Alps, and afterwards by indisposition. The observations on the 30th August were made by an observer whose accuracy may be depended on. However, this gap can have no perceptible influence. Neither can it have been prejudicial to the accuracy of the results, that the observations were not always made on the same day of the month, which was rendered impossible by my other duties and by unfavourable weather. The object of these observations was not so much to ascertain with the utmost exactitude the mean temperature of the places, as to learn the mean difference of temperature between two places situated at different heights.

The observations which I made at the same time at these two spots, extended from October 1835 to February 1836. From this time till April they were unavoidably interrupted, in order to sink the shaft deeper, but after April they were continued. The slight difference of two feet below the surface cannot have any considerable influence on the results.

		Four feet deep in the shade.	Six feet deep in the sun.	Difference.
1835.	October 20.	53°.82	54°.72	—0°.9
	November 4.	51 .35	51 .01	+0 .34
	... 20.	45 .95	44 .60	+1 .35
	December 2.	46 .85	44 .82	+2 .03
	... 21.	43 .25	42 .05	+1 .20
1836.	January 13.	40 .10	38 .75	+1 .35
	February 14.	41 .00	38 .86	+2 .14
	April 12.	44 .15	43 .81	+0 .34
	May 16.	48 .20	48 .20	0
	... 23.	49 .77	50 ·67	—0 .9
	... 30.	49 .89	51 .57	—1 .68
	June 6.	50 .90	52 .14	—1 .24
	... 13.	52 .14	53 .38	—1 .24
	... 20.	54 .05	55 .40	—1 .35
	... 27.	54 .95	56 .75	—1 .8
	July 4.	56 .07	58 .21	—2 .14
	... 11.	56 .97	60 .12	—3 .15
	... 18.	57 .65	61 .02	—3 .37

The difference, therefore, at the end of October and in the middle of May is 0; but from the end of October till the middle of May, the temperature of the place in the shade is constantly higher than that of the place exposed to the sun; and from May till October it is constantly lower. Whether the yearly mean temperature of the two places is the same, or whether, as we should be inclined to conclude from the above observations, the mean of the place exposed to the sun is higher than the other, must be decided by a series of uninterrupted observations. At any rate, the difference does not seem to be great, and it will, therefore, be of little consequence, if, in making observations on the temperature of the soil at various heights, the position of the places of observation with regard to the sun be entirely disregarded. However, should the localities permit, it would always be advantageous at each station to choose two spots for observation, the one in the shade, the other exposed to the rays of the sun.

I am continuing my observations on the *Löwenburg*, but confine myself to the seasons in which the maximum, minimum, and medium take place.

	Bonn.		Löwenburg.	Difference between *a* and *c*.	Difference between *b* and *c*.
	4 feet deep in the shade. *a*	6 feet deep in the sun. *b*	*c*		
1836. June 13.	52°.13	53°.37	45°.95	6°.18	7°.42
July 11.	56 .97	60 .12	50 .45	6 .52	9 .67
August 28.	58 .03	60 .68	51 .46	6 .60	9 .22
October 16.	54 .83	55 .62	50 .00	4 .83	4 .61

Comparing these observations with those made at the same season in 1835, we find, 1*stly*, that in June 1836, the temperature was considerably behind that of 1835, which was a consequence of the cold winter and spring; 2*dly*, that in July 1836, the temperature already exceeded that of July 1835, which must be attributed to the month of June having been exceedingly warm; 3*dly*, that in 1835, the greatest difference of temperature between the upper and lower station took place in June, whilst in 1836, it did not happen till July; and that, in the latter case, it was 0°.9 lower than in the former; and, 4*thly*, that the differences of temperature between *b* and *c* were much greater than between *a* and *c*.

In order, in like manner, to discover the decrease of temperature in the soil to still greater heights in our latitudes, I arranged, on the 30th August 1835, a similar apparatus on the *Faulhorn*, which rises to a height of 8178 feet, and the spot I selected was on the south side of the inn; and on the following day I found the temperature of the water in the bottle to be 36°.51. However, no weight can be attached to this observation, as within twenty-four hours the disturbances caused in the temperature by digging the hole, could not yet have subsided. On the 22d September the brother of Mr Ziegler of *Grindelwald* observed a temperature of 37°.62. More reliance may be placed on this observation, which was made twenty-three days after the burying of the bottle. As we find by interpolation, that the temperature of the soil on the 22d September was, on the *Löwenburg*, 6°.79, and at *Bonn* 7°.19, higher than the mean, the mean temperature on the *Faulhorn* must be between +30°.84 and +30°.45, supposing its temperature to follow the same progression as at those two places.* But these values depend on suppositions which are still to be proved· I, therefore, request all natural philosophers and other travellers accustomed to such observations, who may happen to visit the *Faulhorn*, so celebrated for the grandeur of its scenery, to devote a few minutes to a similar observation, and to have the goodness to communicate to me the result. For this purpose I have committed a thermometer to the care of the innkeeper on the *Faulhorn*, Hans Bohren, of which the observer may make use. The landlord will have the kindness to direct the observers to the spot where the box is buried, and will draw out the bottle. Those who ascend the *Faulhorn* and *Grindelwald*, as is usually done, may make further inquiries of the pastor, Mr Ziegler, who, as I have before said, is making similar observations in his neighbourhood. Of course it is not to be hoped that observations will be made in every month of the year, as the innkeeper himself quits the *Faulhorn* at the end of October, and does not return there till May, and as, in general, during the winter months, it is almost if not quite impossible to ascend this mountain. But even if we only obtain observations in the summer months, we may still hope to succeed in deducing from them the probable scale of the yearly variations of temperature, and from this to obtain the yearly mean temperature with more exactness, than from that single observation. It is to be expected that the yearly variations of the temperature of the soil on this mountain, the height of which is equal to that of the limits of perennial snow in the Alps, are enclosed within narrower bounds than in the valleys. The temperature at the depth of four feet, will no doubt, at this height, fall in winter below 32°. If, therefore, the water in the bottle should have frozen during last winter and broken the bottle, it would be a sign that the temperature had really sunk considerably below 32°. Besides the three points where Mr Ziegler has observed the temperature of the soil since September 1835 (Chap. XVII. p. 160.), he chose two others in April 1836 for the same object. The one is near the bridge over the *Mühlbach*, on the usual road to the *Great Scheideck*, about a quarter of a league from the parsonage-house; the other is near the highest winter habitation in the whole valley, about half a league further up. The former may be about equally distant from the extremities of the two glaciers; the latter is situated somewhat nearer to the upper glacier. Their heights Mr Ziegler estimates at about 300 and 700 feet above the church at *Grindelwald*, but he will endeavour to ascertain it by barometrical measurements. So that we shall within a year receive a series of observations from the environs of *Grindelwald*, which will sufficiently explain to us the extraordinary thermometrical relations of that neighbourhood. If we also succeed in obtaining an approximative value for the mean temperature on the *Faulhorn*, we shall know the decrease of temperature from *Grindelwald*, 3237 feet[*] above the sea, to the summit of this steep mountain, which reaches the limits of perpetual snow.[†]

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* It is, however, probable that the maximum on the *Faulhorn* happens later than at those two places, which are situated so much lower, and, in this case, the mean temperature would there fall still further below 32°. The calculations which follow below, and that which we learn from Heer (Fröbel und Heer loco citato, p. 322) on the duration of the warm season in the various regions of the Alps, speak in favour of this conjecture.

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[*] According to Michaelis, 3265 feet. Fröbel und Heer, loco cit. 262.

[†] But even these observations will not be sufficient to lead to any general conclusions which would obtain at least for the temperate zones. The desired end can only be attained by observing in various latitudes and under quite different climatic and local circumstances. I have reason to hope that observations on the decrease of temperature in the soil will be undertaken at other places in the Alps, in various mountainous districts of *Germany*, in *Sweden*, and even in the *Carpathian* mountains. At some places they are already in progress. Observations in the *Carpathian* mountains will perhaps decide the question, whether the decrease of temperature is there less rapid, as no perpetual snow is found on them, although their height is such that it might be expected to be found there. Still more interesting would be observations on the *Himalaya* mountains, where the height of the limits of perpetual snow present so great an anomaly; V. Humboldt supposes it on the

Before we arrive at this knowledge, it may be ascertained whether probably correct results will be obtained, if we suppose the mean decrease of temperature in the soil, found from Boussingault's observations between the tropics up to the snowy regions, and which so nearly coincides with the result of my observations up to the height of 1173 feet, to be the same for the whole height up to the snowy regions on the Alps in our latitudes. In the following table I have calculated the height, at which the mean temperature will be 32°, from the known mean temperatures of several elevated spots, for the most part in the vicinity of the Alps, always supposing 2°.25 difference of temperature to correspond to 677 feet difference of height.

PLACES.	Geographical Latitude.	Mean Temperature, Fahrenheit.	Height above the sea, Paris feet.	Height above the place where the temp. = 32°.	Height above the sea where the temp. = 32°.
Lowenburg near Bonn,	51° 0′ N.	46°.38	1370	4329	5723
Munich,[1] . .	48 10	47.84	1626	4766	6392
Tegernsee,[2] . .	48 10	45.34	2262	4013	6275
Peisenberg above Munich,[3] .	47 47	42.84	3090	3260	6350
Basel,[4] . . .	47 33	49.26	822	5193	6015
Zürich,[5] . .	47 23	47.95	1254	4799	6053
Bern,[6] . . .	46 57	45.97	1689	4204	5893
St Gothard,[7] . .	46 30	30.11	6438	—569	5869
Geneva,[8] . .	46 12	49.55	1191	5281	6472
St Bernhard,[9] . .	45 51	30.04	7668	—589	7079

[1], [2], [3], [5], [7], [9], Kamtz, Lehrb. d. Meteorol. ii. p. 88.
[4] See Chap. VI.
[6] Kastner's Archiv, xv. 144.
[8] See Chap. XIX.

With the exception of the *Löwenberg* (which lies 3° to 5° further to the north than the rest of the places) and *St Bern-*

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northern side of the chain to rise to a height of 15,900 feet. However, I hope to prove further on, that the law of the decrease of temperature in the soil is not necessarily disturbed by differences in the height of the limits of perennial snow.

I am at present engaged in making maximum and minimum thermometers which shall not be subject to the accidents of the usual ones. Should this succeed, and the experiments yield favourable results, these thermometers would considerably facilitate observations in uninhabited heights and such as are difficult of access. If, for example, they were to be buried four feet deep and left for a whole year, the yearly maximum and minimum would be sufficient to find the mean with the required exactness. In this manner it would even be possible to ascertain the mean temperature on the top of *Mont Blanc*, by burying maximum and minimum thermometers deep in the perpetual snow.

hard, the heights correspond tolerably well for all these places. On the average, then, the mean temperature would become 32°.0 at the height of about 6165 feet above the sea, for places situated between 48° 10′ and 46° 12′ north latitude, and at the height of 8200 feet, the supposed limit of the snowy regions in the Alps, the mean temperature would be +25°.25. This nearly agrees with the observations of Pictet, according to which the snow first shews itself where the mean temperature is +23°.68. But here we find a difference of 9°.67 between the mean temperature on the limits of perennial snow on the equator, as taken by Boussingault from the observations of Humboldt, and that of the limits of the snowy regions in the Alps. This difference, however, is reduced to 7°.42 if we adopt +32°.67 as the mean temperature on the limits of perpetual snow at the equator, which is the value deduced from other observations of Von Humboldt.

As between the tropics the temperature of the soil shews no variations at the depth of one foot, it will shew still slighter variations at the height where the mean temperature is 32°. Snow, which there covers the ground, will therefore be unable to melt on its under surface. But on its upper surface, which is exposed to the air, the snow may partially melt during the summer, because the variations in the temperature of the air are always more extensive than those of the soil, even in the smallest depths, and the temperature of the air at that height between the tropics may, therefore, always rise a few degrees above 32° during the summer. In the temperate zones, on the other hand, the temperature of the soil still suffers considerable variations at that height, in which the mean temperature becomes 32°, and, therefore, rises in summer to several degrees above 32°; supposing the soil not to be covered with glaciers, or any masses of snow by which the communication of heat from above would be prevented. Here, then, snow which covers the ground at that height, must melt away during the summer even on its under surface, and still more on its upper surface, which is exposed to the air, so that it must entirely disappear. This accounts for the possibility of perpetual snow being met with between the tropics, where the mean temperature is equal to or even above 32°, whilst in higher latitudes it does not occur till we reach the regions where the mean temperature is several degrees below 32°, or where the maximum temperature of the soil is 32°.

I have determined the temperature of the soil at the depth of 12 inches, at four elevated spots in the *Swiss Alps;* and in the following table I have added to the results which I obtained, the mean temperatures of the places, calculated upon the principle above established, viz. that the mean temperature is 32° at a mean height of 6165 feet.

Date.	Place of Observation.	Height above the sea in Paris feet.	Temperature observed.	Calculated Mean Temperature.	Difference.
1835, Aug. 27.	At the Mettenberg, on the level of the Eismeer,	5129	44°.37	35°.43	8°.92
Sept. 3.	Spital Matte, .	5887	41.0 *	32.92	8.09
... 3.	Gemmi, . .	6987 †	38.52	29.28	9.25
Aug. 30.	Faulhorn, .	8178	35.24 ‡	25.32	9.92
			36.56 ‖	25.32	1.12

At places in low situations, as *Stockholm*, *Abbotshall*, and *Brussels*, the maximum temperature of the soil at the depth of one foot takes place in July. But if, as my observations on

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[*] In a cleft in the rock the temperature, as low down as I could reach with my arm, was 40°.55, and the same was at a depth of 12 inches in the ground in a place exposed to the full power of the sun, I found a temperature of 41°.45. If the temperature of the numerous fresh-water springs which rise in that neighbourhood (Chap. IV.) give the true mean temperature of the soil, this must be +37°.40. But these springs which rise out of the much-fissured rocks of limestone are probably thermal (Chap. I.), even though they proceed from the *Daubensee* (Chap. XI.)

[†] According to Michaelis (Fröbel und Heer, p. 272) 7049 feet; according to others 6953 to 7160 feet.

[‡] This is the mean of eighteen observations on the summit of the mountain, which were made every half-hour from 9½ A. M. till 6½ P. M., and at 5½ A. M. of the 31st August. The maximum was 35°.60, and the minimum 35°.15.

[‖] This is the mean of the observations which were made at the same time as the others, but 40 feet below the summit on the southern declivity of the mountain. The maximum was 37°.40, the minimum 36°.27. Both series of observations were made at a time when the *Faulhorn* had been already some days covered with snow one foot deep. The places where the observations were made were, however, freed from the snow.

the *Löwenberg* give reason to suppose, the maximum at these more elevated points happens somewhat later, then it is probable that the observations made at the end of August and the beginning of September were already somewhat under the maximum.

The maximum, therefore, probably exceeds the observed temperatures at those places by 2°.25, perhaps more, and thus we should have at least 43°.25 for the maximum temperature of the soil on the *Spital Matte*, which is situated nearly at the height in which, according to our hypothesis, the mean temperature is 32°. But where the maximum temperature of the soil reaches 43°.25, no perpetual snow can be found. Even on the top of the *Faulhorn* the maximum is probably above 37°.62, so that here also perennial snow cannot be met with. Yet the summit of this mountain rises as high as the limits of perennial snow in the Alps are supposed to exist. However, these limits are supposed to rise on single insulated peaks, to a height of 8400—8700 feet, and on extensive chains of mountains to fall to 7800 feet.*

Besides the observations in the Alps, we have also those made at *Dresden*, *Freyberg*, and at several other places in the *Saxon Erzgebirge*, in the years 1830–1833, which furnish us with some data on the decrease of temperature with the height. For 2°.25 difference of temperature they give

Between	A Difference of Height	
	According to the arithmetical mean.	According to the the method of smallest squares.
Dresden and *Freyberg*,	891,5	885,7
Dresden and four different places in the *Erzgebirge*,	770,7	680,2
Freyberg and three different places in the *Erzgebirge*,	677,5	640,2

From the thermometrical observations of the soil near the surface, made at various heights in the *Erzgebirge*, Reich calculated that the decrease of temperature in the soil there amounts to 2°.25 in 744 feet on the average. This result cannot be compared with the above, as the temperatures of the air and of the soil were not made at the same places, but at places very different in their climate; further, because the mean temperature at *Altenberg* in the years 1830–1832 shewed differences as great as those between that place and *Markus Röhling;* and lastly, because the observations on the temperature of the soil at *Altenberg* and *Johanngeorgenstadt* even shewed an increase of temperature with the height.

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* Ebel, Part II. p 49.—On the top of the *Seidelhorn*, which is 8580 feet high, I found no snow on the 18th August 1822; but in a ravine on the eastern side of the mountain, a considerable mass of snow was accumulated.

However, these observations seem also to prove that the decrease of temperature is slower in extensive gradually ascending mountains, but more rapid in steep ones. As there may, therefore, be an infinite number of gradations in the decrease of temperature between mountains which rise almost perpendicularly from the surface of the earth, and a gradually ascending inclined plane, all other circumstances being the same; only such observations can serve for a comparison, as have been made on mountains having a very steep ascent.

With respect to the decrease of temperature with the height in the frigid zones, we are totally unprovided with observations in those regions, which might serve as data in our calculations. We must, therefore, leave it undecided, whether the relations are the same or different from those which exist in the other zones. If Fourier's hypothesis,* that the temperature of space, or at least of that part of space in which the earth performs its orbit, is about equal to the mean temperature at the poles is correct, no decrease of temperature can be supposed to take place in proportion to the distance from the surface of the earth at the poles.

We now come to the last means of determining the decrease of temperature with the height, namely, the temperature of springs, which seems indeed to be the simplest method of all.

In Chapter VI. it has been shewn, that, under the most favourable circumstances, the mean temperature of the soil can only be determined from such springs as have not a constant temperature, because springs of constant temperature always rise out of strata which are situated much lower. But the mean temperature of variable springs can only be found by means of monthly observations continued for a whole year, or by observing exactly at the time of their mean temperature. When once the time of the maximum or minimum is known, it is easy to determine the time of the mean, for this always happens three months after these extremes.

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* Mém. de l'Instit. 1824, p. 580.

But mountain springs scarcely ever give the true temperature of the place at which they rise, but generally of a more elevated region, that is to say, they bring cold with them from above.—(Chap. IV).

From this it follows, that single observations on springs are of no use for the determination of the mean temperature of the soil, for repeated observations are necessary for deciding whether the springs have a constant temperature or not, and if not, when its mean happens. But it is impossible to discover whether a spring give the true temperature of the place, without the assistance of observations on the temperature of the soil itself. Thus it follows, that even monthly observations of mountain springs, continued for a whole year, cannot be of use for the determination of the mean temperature of the soil.

The following table contains the results of one year's observations made by me on the temperatures of eighteen springs which rise between *Poppelsdorf* near *Bonn*, and the highest points in the *Seven Mountains* (*Siebengebirge*), at which springs are still to be met with. It proves what we have just said most clearly.

The numbers between parentheses were determined by interpolation, but all the rest by observations, made for the most part in the middle of the month, excepting when other duties or unfavourable weather prevented it. As the greater number of these springs shewed but small variations from month to month, it could be of little consequence whether the observations were made a few days earlier or later in the month. The first seven springs rise all nearly on the same level, on the slope of the hills belonging to the lignite formation, which run from *Poppelsdorf* to *Godesberg*. The ground rises imperceptibly from the former of the places to the latter.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
	Well in the Chemical Laboratory near Bonn.	Spring near Poppelsdorf.	Spring near Poppelsdorf.	Spring between Kessenich and Dottendorf.	Spring near Dottendorf.	Mineral Spring at Godesberg.	Fresh-water Spring at Godesberg.	Spring near Röhndorf, on the road to the Löwenburg.	Spring at the Quekstein, above the Wintermühle.	Spring at the Quekstein, under the Wintermühle.	Spring at the Quekstein, out of a crack in the sandstone.	Spring at the Kunterbrunnen.	Spring on the Bronner Spitze.	Spring an the Wolkenburg.	Spring at the Fuchsschwanz on the Stenzelberg.	Spring in the Taubenort.	Spring on the Ausrottwiese on the Lorberg.	Spring on the Gierscheid's Wiese on the Lorberg.
Height above Königswinter in Paris feet,								28	161	161	161	282	352	352	465	551	599	752
1834, August, .	°	°	°	51°.12	53°.15	°	°	°	°	°	°	°	°	°	°	°	°	°
September,	51.8	52.25	52.25	51.35	54.05	52.70	53.15	52.47	50.00	50.9		51.35	50.22	51.35	50.67	48.42	54.50	48.20
October,	51.8	52.25	52.25	51.91	53.15	51.57	50.9	52.70	50.11	50.9		51.35	50.22	51.35	50.0	48.65	54.27	48.65
November,	51.35	48.87	50.22	50.9	50.9	50.45	49.55	52.02	48.20	50.00		50.45	49.66	50.45	48.65	47.75	48.87	46.85
December,	51.23	48.76	50.00	50.56	49.55	49.66	48.42	51.8	48.20	49.79 50.00	48.2	50.45	49.55	50.22	47.75	47.75	47.97	46.62
1835, January,	50.45	48.65	49.55	50.67	48.98	49.10	46.62	51.35	48.20	48.87	48.65	50.22	49.32	49.55	47.07	47.75	47.52	46.28
February,	51.12	48.31	49.10	50.11	47.75	(48.87)	(47.86)	50.9	(47.90)	(48.87)	(48.20)	(49.90)	(48.98)	(49.02)	(46.96)	(46.96)	(46.28)	(45.76)
March, .	50.9	48.20	48.98	49.32	46.40	48.65	49.10	50.67	47.63	48.87 49.10	48.20	49.55	48.65	48.31	46.85	46.17	45.08	45.27
April, .	50.9	48.65	49.32	49.55	46.85	48.65	49.55	50.45	47.75	48.87 49.10	48.65	49.55	48.65	48.65	46.85	46.40	45.72	45.27
May, . .	51.12	49.55	49.79	49.55	47.30	49.79	50.67	50.45	48.20	49.32 49.55	49.55	49.79	48.87	49.10	47.52 48.2	45.95 46.85	45.72 48.20	45.50 45.95
June, . .	51.35	50.22	50.45	49.66	49.10	50.45	50.9	50.67	48.65	49.55	50.00	50.00	49.32	49.43				
July, . .	51.35	51.8	51.35	50.22	51.35	51.35	51.8	51.23	49.32	49.79 50.00	50.33	50.00	49.32	50.00	48.87	47.30	50.45	46.62
August, .	51.46	52.45	51.8	50.67	52.25	51.35	51.35	51.57	49.10	49.79 50.00	50.67	50.11	49.55	50.22	49.1	47.52	52.25	46.73
September,								51.8										
Mean, .	51.23	50.0	50.41	50.39	49.90	50.22	50.02	51.35	48.58	49.60 49.73		50.22	49.37	49.81	48.20	47.30	48.89	46.52
Differ. between Max. and Min.	1.35	4.05	3.26	2.58	7.65	4.05	4.92	2.25	2.36	2.02		1.8	1.57	2.81	3.82	2.47	9.45	3.37
1835, October,								54.05						50.45			49.77	
November,								51.57						50.0			47.75	46.62
December,				49.55	48.65			50.9						49.10			45.08	45.95
1836, January,								50.45						48.20			45.08	45.08
February,								50.00						48.20			46.85	45.08

No. 1 is an artificial well 58 feet deep, which, like all other wells in this neighbourhood, is supplied with water from the *Rhine*. Its high mean temperature compared with that of the six following ones, is evidently caused by the increase of temperature towards the interior of the earth. The mean temperatures of 2—7 agree, however, tolerably well together, and the mean of these six might therefore be taken as the mean temperature of the soil between *Poppelsdorf* and *Godesberg*. However I exclude No. 5, because it rises from very near the surface, as is plain from its more extensive variations of temperature compared with the others, and because it evidently receives its water from the neighbouring hills. In this manner, then, we obtain a mean of 50°.22. The springs 8—18 are all situated on the other side of the *Rhine*, in the *Siebengebirge*, and rise by degrees above No. 1, as the annexed numbers indicate. It is very remarkable that No. 8 has a mean temperature 0°.112 higher than No. 1, although situated considerably higher. But if we examine its localities more closely, it no longer appears astonishing; for it rises at the foot of the *Drachenfels*, on the eastern side, where the mountain is unusually steep; and if it runs but a few hundred feet in a horizontal direction, or at least in a direction nearer to the horizontal than the slope of the hill, the furthest point of its course may easily lie 100 feet beneath the surface of the mountain. It is, therefore, probable that it rises from a greater depth, in comparison with the mountain, than any one of the rest of the springs; it consequently brings a higher temperature with it from the interior of the mountain, and must be looked upon as thermal, although it certainly does not rise from a greater depth by means of hydrostatic pressure. I will pass over No. 9, because this spring runs for a considerable distance on the surface, and I could not trace it to its source. No. 10 presents a curious phenomenon, namely, when I observed its temperature in December, I found, by plunging the thermometer in at different parts of the little natural basin which it forms, a difference of 0°.225, and remarked that there are two small springs which rise here, scarcely six inches apart, with different temperatures. The lower numbers shew the differences of temperature between the two. I then discovered another spring, No. 11, scarcely three paces distant from the last, which issued from a cleft in the rock among loose blocks of stone, in a place which was difficult to be got at, and this shewed 1°.35 to 1°.575 lower than No. 10. These three springs rise out of the sandstone of the lignite formation, which is there very much fissured. The comparatively higher temperatures of No. 12 and 14 was no doubt occasioned by both rising from a greater depth, and appearing in deep ravines. The former evidently rises from a greater depth, as may be seen by the motion of the sand at the bottom of the basin. When springs rise in deep ravines from below, they must always appear with a temperature superior to the mean of the place where they issue. All the rest of the springs either run down directly through cracks in the rocks, or at least bear evident signs that they do not rise from below.

If we take all these circumstances into consideration, and compare the mean temperatures with the respective heights, it becomes evident that these mountain springs are by no means capable of giving the decrease of temperature in the soil with the height. If, for instance, we compare the mean temperature of Nos. 3 and 18, we find that the scales of their annual variations are nearly about the same; it may, therefore, be supposed that they both rise from about the same distance below the surface. But, notwithstanding that they seem to be so peculiarly adapted to the determination of the relative decrease of height, yet when we come to examine into it we find it to be exactly the contrary: for the decrease of temperature calculated from this mean is 2°.25 in 428 feet; whereas the same decrease is found, from the direct observations made near *Bonn* and on the *Löwenberg*, to be 2°.25 in 683 feet. It is clear, then, that the spring No. 18, which issues from a cleft in the rock, brings down cold with it from above, and, therefore, does not shew the temperature of the place where it makes its appearance.

Although my endeavours to determine the decrease of temperature with the height, from the mean temperatures of mountain springs, have been unsuccessful, yet we see that the springs in the valley of the *Rhine*, which lie at about the same level, are very well adapted to the determination of the mean temperature of the soil. It is true their mean temperature, 50°.22

differs by 0°.787 from the above-mentioned mean obtained from the direct observations on the temperature of the soil; but, as the observations on the springs occupied the space of time from August 1834 to August 1835, and therefore included the mild winter of 1834–35, whilst the observations in the soil lasted from February 1835 to February 1836, and included the severe winter of 1835–36, we cannot be surprised at this difference. And, in fact, the observations on the temperature of the soil shew a considerable fall from October 1835 to February 1836.* My observations communicated in Chap. VIII., so far as they can be reckoned upon, are also in unison with this. But the necessity of observing the rule, already laid down in Chap. VI., never to attempt to determine the mean temperature of a place but from *several* springs, is rendered obvious by comparing the spring No. 1 with the six following ones. The less the extent of the variations of temperature in a spring is, the more probable is it that it brings up heat with it from below; and, indeed, we find the extent of variation in Nos. 1–7 to decrease in proportion as the mean temperature increases.

For the reasons explained in Chap. VIII., it may with probability be assumed that the springs Nos. 5, 7, and 17 rise from a depth of about twenty-four feet, but the rest from a greater depth. Nos. 8, 10, 12, and 13, rise without doubt from the greatest depth, which may easily amount to 60 feet, when we compare their scales of variation with that of No. 1. The comparatively high temperatures of Nos. 8 and 12, of which we have already before made mention, are easily accounted for, when we consider that they rise in deep ravines, so that the rocks rise steeply and to a considerable height on both sides. If these do actually rise from a depth of sixty feet, they may very easily acquire a mean temperature of 1°.125 higher than that of the surface, by reason of the increase of temperature towards the interior of the earth.

If the depth from which springs rise could be ascertained, the mean temperature of a spring would exactly give the mean temperature of the place of its origin, so long as it be not a mountain spring, which brings down a low temperature from above. Indeed, if this depth were known, a single observation at any season of the year would be sufficient, if the scale of the yearly variations in the temperature of the soil at the same depth were also known. Running springs, which are conducted through pipes for a long distance at a known depth below the surface of the earth, would be very well suited to this purpose, provided they did not descend from steep mountains. From this we see how desirable it is, that observations on the temperature of the soil at various depths, at different places, and at different heights above the sea, should be multiplied as much as possible.

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* The mean temperature of the air at *Bonn* is not yet accurately determined. But the mean of the Medical assessor Mohr's fourteen years' observations at Coblentz, from 1818 to 1831, corrected according to the rule in gartner's Naturlehre, 3d edit. p. 697, is 51°.12.

The continuation of my observations in some of the above springs, after the completion of the one year's series, gives us an idea of the influence of the temperature of the air on that of springs; for if we find by interpolation the temperatures of these springs on the same days in 1834–1835, on which they were observed in 1835–1836, we have:

	By Interpolation.		Difference.
	1834	1835	
No. 8.—Spring near Röhndorf,			
September 2. . .	52°.44	51°.8	0°.631
October 20. . .	52.536	52.25	0.271
November 8. . .	52.149	51.57	0.574
December 20. . .	51.72	50.9	0.821
	1835	1836	
January 10. . .	52.25	50.45	0.99
July 12. . .	50.67	50.45	0.225
July 10. . .	51.23	50.9	0.337
No. 14.—Spring on the Wolkenburg, .	1834	1835	
October 20. . .	51.12	50.45	0.675
November 8. . .	50.619	50.00	0.619
December 20. . .	50.11	49.10	1.01
	1835	1836	
January 10. . .	49.68	48.42	0.256
June 12. . .	49.52	49.10	0.418
July 10. . .	50.00	49.55	0.45

TABLE—*continued.*

	By Interpolation.		Difference.
	1834	1835	
No. 17.—Spring on the Ausrottwiese.	°	°	°
October 20. . .	52.929	49.79	3.19
November 8. . .	49.856	47.75	2.106
December 20. . .	47.896	45.05	2.85
	1835	1836	
January 10. . .	47.61	45.05	2.56
June 12. . .	47.56	45.95	1.613
July 10. . .	50.45	48.65	1.8
No. 18.—Spring on the Gierscheld's Wiese.	1834	1835	
November 8. . .	47.18	46.62	0.337
December 20. . .	46.546	45.95	0.391
	1835	1836	
January 10. . .	46.35	45.05	1.305
June 12. . .	45.83	45.16	0.225
July 10. . .	46.62	45.95	0.675

We here find that the temperatures of these springs in the months of October, November, and December 1835, and January 1836, were altogether lower than during the same months of 1834 and 1835. This lower temperature still shewed itself even in June 1836. In general, these differences all increased as the observations went on, and were greatest in January. But as no observations were made between January and June, it is impossible to decide whether the differences continued to increase still further in the following months. However, the observations in June prove that they afterwards decreased again, and it may be expected that in the following months they will altogether disappear, or perhaps even become negative, if the summer turns out very warm. The maximum differences in the springs Nos. 8, 14, and 18 (0°.99, 1°.26, and 1°.30), are nearly exactly in proportion to the differences between their yearly maxima and minima (2°.25, 2°.81, and 3°.37), as given by the above series of observations, from August 1834 to August 1835. In other words, the influence of the external temperature on springs, is smaller, the smaller the yearly variations in the temperature of the springs are, that is to say, the greater the depth from which they rise. However, not one of these springs rises from a depth sufficiently great for the influence of the external temperature to be entirely without effect. No. 1, the well under the chemical laboratory, alone seems to be no longer affected by it, because its temperature is almost constant. On the 13th June 1836, I found its temperature to be the same, 51°.23 as in 1835. Lastly, No. 17, the one which shews the most considerable variations of temperature of all, and therefore takes its rise very near the surface, also shews the greatest differences between the two years; and it is very plain how much it was affected by the cold weather which set in at several periods of the winter of 1835–36.

In order to have a numerical value of the influence of the temperature of the air on that of springs, I subjoin a table of the mean temperatures of the following months of the years 1834, 1835, and 1836, according to the observations of Mr Heis at *Cologne*.

	Mean Temperatures.		Difference.
	1834,	1835,	
June,	65°.90	64°.96	+0°.92
July,	71.60	69.91	+1.68
August,	68.76	66.70	+1.05
September,	60.35	61.92	—1.57
October,	52.92	51.35	+1.57
November,	43.47	38.97	+4.5
December,	39.87	34.02	+5.85
The whole Year,	52.58	51.46	+1.95
	1835,	1836,	
January,	38.75	35.60	+3.15
February	41.22	36.72	+4.5
March,	42.82	46.49	—3.64
April,	49.55	50.22	—0.675
May,	56.11	57.89	—1.70
June,	64.85	67.55	—2.7
July,	70.02	68.9	+1.125

The year 1835 was, therefore, with the exception of September, always colder than 1834, and hence the depression of the temperatures of the springs in 1835. The higher temperature in September, was distinctly shewn in the temperatures of the springs in October and November.

Before I had made my own experiments, as above given, on the temperature of mountain springs, I gave myself a great deal of trouble to find out the decrease of temperature in the soil with the height, from Wahlenberg's single observations on the temperature of the *Swiss* Alps,[*] von Buch's in the *Canary Islands*,[†] Forchhammer's on the *Faroë Islands*,[‡] and Kern's on the *Swabian Alps*.[§] But the results of these attempts were as unsatisfactory as Kämtz's calculations[‖] on Wahlenberg's observations, which gave the very improbable result, that at a height of 2700 feet, the temperatures of the air and of the soil are equal, but that at the height of 12,000 feet, the soil is 11°.25 warmer than the air.

I have myself observed the temperature of a great number of springs in the Alps; but it would be of no assistance to us, in our present object, to communicate them here. That which resulted from the one year's observations in the *Siebengebirge*, on a small scale, was there seen on a large scale. For example, in the neighbourhood of *Grindelwald*, namely, at the foot of the *Mettenberg* and of the *Eiger*, I observed no less than twenty-two springs, whose temperatures were between 37°.40 50°.00, and the coldest were precisely those which were on the lowest level. What conclusions can be drawn from these on the temperature of the soil, and on the decrease of temperature?

Chap. XIX.—*What Conclusions may be drawn from the Observations hitherto made on the Increase of Temperature towards the Interior of the Earth?*

In the foregoing chapters we have become acquainted with the various causes which may modify the increase of temperature towards the interior of the earth. Is it then surprising that the results should be so very various, frequently bearing no comparison to each other?

Kircher[¶] seems to have been the first who took notice of the increase of temperature in the earth. The mining officers of *Schemnitz* remarked to him that they had nothing to suffer, either from heat or cold in the mines, so long as there was a good circulation of air; but when such was wanting, it invariably became warmer. From Mr Schapelman, the director of the mines (Bergmeister) at *Herrengrundt* in *Hungary*, Kircher learnt that when the mines are dry, their temperature increases with the depth, because it is impossible, on account of their depth, to give them a sufficient ventilation; but that when they contain water, although deep, they are not so warm. Boerhavé[*] also mentions the increase of temperature towards the centre, and Mairan[†] first set up the hypothesis of the existence of fire in the interior of the earth.

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[*] De Veget. et Clim. in Helvet. septentr., p. lxxvii. § 87.

[†] Poggendorff's Annal. xii. 407.

[‡] Karsten's Archiv für Mineralogie, &c. &c. ii. 199. See also Poggendorff's Annal. xxxv. 213.

[§] Kastner's Archiv für Chem. und Meterol. iv. 42.

[‖] Lehrb. der Meteorol. ii. 200.

[¶] Mundus Subterraneus, 1664. fol., vol. ii. p. 184 and 185.

Regular observations of temperature in mines began in the middle of the last century, and have been very much multiplied since that time. Either the temperature of the springs, or that of the water which flowed from the adit, was usually observed; but the number of the observations was inconsiderable, and the results are themselves very uncertain, since the waters may bring cold from above or heat from below with them[‡]; or else they observed the temperature of the air on the rock. We possess observations of this sort in great numbers, and in depths of 1278 feet 11 inches, 1598 feet 8 inches (1200 to 1500 feet French feet ?).

The first observations were made towards the middle of the last century in *France*, at the instigation of Mairan,[§] in the mines of *Giromagny*, near *Befort*, in the *Vosges*. In the year 1806, D'Aubuisson[||] made some observations in the lead and silver mines of *Poullaouen* and *Huetgoët*, in *Bretagne;* and from 1822 to 1825, Cordier[¶] did the same, in the coal mines of the departments of *Tarn*, *Nièvre*, and *Calvados*.

From *Switzerland*, we have the observations of De Saussure,[**]

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[*] Elementa Chemiæ, Lipsiæ, 1732, vol. i. p. 403.

[†] Mém. de l'Academie pour 1719.

[‡] The former case seems to be the more frequent.

[§] Dissertation sur la glace, à Paris, 1749, in 12mo, p. 60.

[||] Journ. des Mines, vol. xxi. p. 60.

[¶] Annal. des Mines, deuxième series, vol. ii. p. 53–139.

[**] Voyages dans les Alpes, § 1088.

which he made about fifty years ago, in the salt-mines of *Bex*, under very favourable circumstances.

Many observations were also made in *Germany*, namely, in *Saxony*. As, for example, those of Von Freiesleben and Von Humboldt, in the year 1791;[*] those of D'Aubuisson, in 1802[†] of Von Trebra in 1805, 1806, 1807, and 1815,[‡] and lastly, those of Von Herder, under the special management of Reich, in the years 1830–1832.[§] In 1828–1830, regular observations have been carried on in various *Prussian* mining establishments.[||]

Great Britain has afforded us a great number of observations since 1815; namely those of Thos. Lean, Rede, Forbes, and particularly W. Fox and Moyle in the copper and lead mines of *Cornwall* and *Devonshire*, and those of Bald, Dunn, and Fenwick, in the coal-mines in the north of *England*.[¶]

In *Sweden* observations were conducted by the director of mines, Claes Wallmann, in the mines of *Fahlun*.[**] From *Italy* we have those of Fantonetti, in the gold mines of *Pestarenadi-Macugnana*,[††] in the valley of *Anzasca*.

And lastly, we are indebted to Von Humboldt for observations in several of the mines of *Mexico* and *Peru*.[‡‡] Complete compilations of these observations, with the exception of those more recently made in *Saxony* and *Prussia*, are to be found in various works.[§§]

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[*] Annal. de Chim., vol. xiii. p. 210.

[†] Description des Mines de Freyberg, vol. iii. p. 151, 186, and 200, Journ. des Mines, vol. xi. p. 517, vol. xiii. p. 113.

[‡] Geogr. Ephemeriden, 49, 432. Annal. des Mines, vol. i. p. 377, vol. iii. p. 59, Annal. de Chim. vol. xiii. p. 211.

[§] Reich, loco cit.

[||] Poggend. Annal. vol. xxii. p. 497.

[¶] Annal. de Chim. et de Phys. vol. xiii. p. 200, vol. xvi. p. 178, vol. xix. p. 438, vol. xxi. p. 308, and Geogr. Distribution of Plants, by Winch, p. 51.

[**] Kongl. Vetensk. ac Handl. ar 1831.

[††] See Brugnatelli's Giornale, 1821, p. 467; also Gehler's neues Phys. Wörterbuch, vol. iii. p. 978.

[‡‡] Annal. de Chim. et de Phys. vol. xiii. 207.

[§§] Annal. des Mines, vol. i. p. 378, and vol. iii. p. 63. Annal. de Chim. et de Phys. vol. xiii. p. 183. Gilbert's Annal. vol. lxxvi. p. 390, very complete. Gehler's neues Phys. Wörterbuch, vol. iii. p. 974 and following. Reich, loco citato, p. 139, very complete and comparative.

The following is borrowed from Cordier's comparative results of these observations.

The mean depth in which the temperature rises, 2°.25 F. or 1° R. is, 1st, From the observations of the temperature of the springs; in *Saxony*, 188 feet 7.7 inches (English feet); in *Poullaouen*, 450—10; in *Huetgoët*, 122—6; in *Cornwall*, 102—4; in *Mexico*, 102—4. 2d, From the observations on the temperature of the workings of the mines: in *Cornwall*, 69—3; in *Devonshire*, 110—10; in *Bex*, 106—6.9; in *Poullaouen*, 262—2. 3d, From the observations on the temperature of considerable bodies of water in the mines: in *Cornwall*, 118—4; in *Saxony*, 139—7; in *Huetgoët*, 175—10. 4th, From observations on the temperature of the rock in mines; in *Saxony* 198—3; in *Cornwall*, 69—3 to 122—6.

Cordier's own observations give a mean of 147—10.2 and 61—9.7 feet. As a general result, he asserts that the increase in the internal heat of the earth does not every where follow the same law; that it may be twice or even three times as rapid in one country as in another; that these differences are totally independent of latitude and longitude, and that the increase is certainly greater than has been generally supposed. According to him, an increase of (1° R.) 2°.25 may take place in 61—9.7, or even 53—3.4 feet; but for the present he takes it at 102—3.7. Kupffer[*] attempts, but with little success, to reconcile the irregularities in these results. Still less can we agree with him, in wishing to determine the mean temperature of the soil from the increase of temperature observed at any particular place.

According to the above-mentioned observations in the mines of *Prussia*, the calculated depth in which the temperature increases 2°.25 F. (1°R.), varies from 63—11.3, 473—5.4 feet.[†] The latest observations in the *Erzgebirge* also yield very various results; indeed, in the *Neue Hoffnung Gottes* at *Bräunsdorf*, and in the Stockwerk at *Altenberg*, the temperature is observed in some cases to decrease. Reich, therefore, calculated the most probable mean from all the results, by taking into consideration, as far as he was able, all the causes which might have influenced the observations. In this manner he found, for an increase of temperature of 2°.25 F. (1° R.), a depth of 171—6.9 feet. In order to judge of the probability of a constant error, he examined whether the warming or the cooling influences predominated. He found that the error was on the side of the latter, and that the increase of temperature was consequently found too small, which seems also to accord with the observations in other places. Although in several mines observations have been made at more than two points, one below the other, it cannot be determined whether the increase of temperature is proportional to the depth. Sometimes the temperature increases, sometimes decreases with the depth; sometimes it decreases at first, and afterwards increases again, and *vice versa.*

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[*] Poggend. Annal. vol. xv. 171. See also Reich, l. c. p. 149.

[†] Loco citato, p. 528.

The thermometrical observations hitherto made in mines have, then, given such very various values for the increase of temperature towards the interior, that it is impossible from them to discover the law. This is by no means surprising when we consider how numerous are the causes which tend to discompose that law, and which have been treated of in Chapters IX, XI, XII, to XVII. We have already seen, from the reflections in Chap. XVII. what a considerable influence is due to the configuration of the earth. Some of the observations may have been made in mines excavated on the declivity of steep mountains, in which case, as is shewn in Fig. V, Pl. II, a very protracted increase of temperature would take place. A shaft sunk from *C* to *e*, for example, would have given an increase of 2°.25, 2°.65—4°.4 F. (1° R.) in 249 feet, whilst shafts at *c* and *d* would have given 122—6.7 feet. If the configuration of the surface of the earth, at each point of observation, were known, we should be enabled, by means of the formula developed in Chap. XVII., to correct the modifications of the law thereby effected. But how is it possible to bring into calculation the numerous other influences?

However, almost all observations indicate that there exists an inward heat, which is peculiar to the earth itself, which does not proceed from the sun's rays, and increases rapidly with the depth; for the few observations which have given a contrary result cannot be brought forward as proofs against the generality of this assertion, since local circumstances may sometimes, as, for instance, in the *Sauberg* at *Ehrenfriedersdorf* (see Chap. VI.), cause a decrease of temperature to a certain depth. This may, perhaps, also be the case in the *Swedish* mines.[*]

Even the most strenuous opponent must be convinced when he considers the high temperature possessed by considerable bodies of water collected in deserted mines. It is impossible to conceive any other cause for this than the innate heat of the walls of the cavities in which the water accumulates. But the temperature of these walls cannot be ascribed to any other cause than the natural high temperature of the earth at a certain depth. Thus, Fox relates that in 1820 the water in the copper and tin mine of *Poldice*, at a depth of 144 fathoms, had a temperature of 79°.92 F., whilst in 1830, when the workings had been carried to a depth of 176 feet, it rose to 99°.95 F.[†]

A rare opportunity of observing the temperature of an insulated body of water presented itself to Reich.[‡] In a mine near *Freyberg*, a supply of water of several cubic feet per minute was cut off, for the sake of prosecuting the works, at a depth of 916—6.7 feet, by means of a *Keilverspündens*. This was composed of wedge-shaped pieces of wood of 6—4.7 feet long, which fitted so exactly together, and against the smooth-hewn walls of the gallery, that, notwithstanding a pressure of 18 atmospheres, a very small quantity of water could escape; on the 20th of March 1833, the escape was only 0.326 cubic feet in an hour. In this manner a space of 62 feet long, 6 feet high, and 3 feet wide, was shut off and filled with water, which had hardly any communication with the gallery. Reich carefully examined the temperature of this body of water on the 30th September 1832, and on the 20th March 1833. These seasons were chosen, because, if the influence of the seasons should be perceptible, it would on these days be the most striking. On the 30th September, he found the temperature of the water, from five perfectly accordant observations, to be 61°.65, and that of the air, 6 fathoms from the *verspünden*, 60°.8 to 61°.02; and on the 20th March, also from five perfectly accordant observations, the temperature of the water 61°.56, and that of the air, at the same place as before, 60°.23. The difference of 0°.09 is within the error of observation. It is certain that the enclosed body of water must have assumed the temperature of the surrounding rock; and in Reich's opinion there can be but two causes which could occasion a difference between them. The first is the escape of some of the water through the *verspünden*, and its replenishment from above; the second is the continued change of air before the *verspünden*. But both of these influences can be but inconsiderable, for, as we have already remarked, a very small quantity of water makes its escape, and the enclosed water is separated from the air by a mass of wood of 6 feet in thickness. Further, these two circumstances could only tend to diminish the temperature of the water, as the water was replenished from above, and the air at the end of the summer, as well as at the end of the winter, was colder than the water, although the difference was smaller in the former case than in the latter.

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[*] Bergman, Physikal-Erdbeschreibung, 1780–4, vol. ii. p. 118; and Waldmann in Berzelius' Jahresbericht, vol. i. p. 149.

[†] Poggend. Annal. vol. xxi. p. 171.

[‡] Loco citato, p. 134.

From these observations, Reich calculated an increase of temperature of 2°.25 in 136—11.3 feet, which is very much more rapid than the mean deduced from the observations in the mines of the *Erzgebirge*. This adds some weight to the supposition expressed above, that the mines are by degrees reduced in temperature by the entrance of cold air and water from the surface.

Later observations of Phillips,* in a shaft of unusual depth recently sunk at *Monk-Wearmouth*, near *Newcastle*, gave results which corresponded very nearly with those in the *Erzgebirge*. All external influences could only have caused a depression, but not an elevation, of temperature in this shaft. Without detaining ourselves with the observations on the temperature of the air and the coal, we will only mention, that that of the salt-water which collected in a small shaft in the vicinity, and from which there was an uninterrupted disengagement of bubbles of carburetted hydrogen, was 72°.59, and when the disengagement of gas subsided, 72°.0 and 71°.60. The mouth of the shaft lies 86—10.2 feet above the flood-mark of the sea; the point of Phillips's observations 1584—0.8 feet below the opening of the shaft, and consequently 1497—0.6 feet below the surface of the sea. At the upper end of the shaft, the mean temperature of the air was 47°.58, and that of the salt-water below 72°.59. Supposing the temperature not to have increased in the last 100 feet (?), so that 72°.59 were the temperature at the depth of 1484—0.7 feet, Philips calculates an increase of 2°.25 F. (1° R.) in a depth of 133—6.8 feet.

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* Philosophical Magazine, 1834, vol. v. p. 446.

These observations point out in what manner the most accurate results as to the increase of temperature towards the centre of the earth are to be obtained. In old exhausted mines galleries are often to be found in which water might probably be shut off in a similar manner for the sake of observations, as was done in the *Freyberg* mine above referred to. Although the expense of the observations would be increased by such operations, yet, on the other hand, they would be very much simplified, as it would be sufficient to make them *twice* a-year, in spring and autumn.

Another very favourable opportunity of obtaining accurate thermometrical observations offered itself in the sounding at *Pregny* near *Geneva*, already referred to more than once.

Mr Giroud wished to have an artesian well sunk at his country seat at *Pregny*. On coming to a depth of 582—11.7 feet. without meeting with water, he gave up the undertaking. Messrs de la Rive and Marcet upon this opened a subscription for the continuation of the work, and thus they succeeded in reaching a depth of 726—10.2 feet. The failure of the first object of the undertaking, namely, to obtain spring water, was particularly favourable to the observations, as the variations of temperature, caused by the gushing in of water into the well, were thereby entirely avoided. The water in the well rose once to within 13—10.2 feet of the surface; at 106—6.9 to 159—10.3 feet deep it was so muddy that no such influx of water could possibly have been supposed to exist, especially near the bottom it was rather a very wet earth than water. The prejudicial effects of currents of air in dry wells were consequently also prevented. Besides which the thermometer was enclosed in a cylinder of 4½ inches in diameter, which exactly fitted into the well, so that, at least, whilst it was sunk to any certain depth for ascertaining the temperature, all possible influx of water was prevented. Lastly, the fact that the results obtained at a certain depth exactly coincided, whether observed at the bottom of the well or at the same point afterwards, when the work had been carried on to 106—6.9, 213—1.8, or 319—8.7 feet below that point, puts it beyond doubt that nothing could have tended to disturb the observations.

The temperature was observed with the utmost care by means of a maximum thermometer. The results shewed so exact a coincidence among each other, as well as with those of a common thermometer in the less considerable depths, that the greatest confidence may be placed in them. The following includes the whole series:—

FIRST SERIES.		SECOND SERIES.	
Depth below the surface.	Corresponding Temperatures.	Depth below the surface.	Corresponding Temperatures.
Feet.	Fahr.	Feet.	Fahr.
31—11.7	50°.9	319—8.7	
63—11.3	51.12	63—11.3	
106—6.9	51.8	106—6.9	51°.57
159—10.3	52.47	151—4.1	52.43
213—1.8	53.37	213—1.8	53.15
266—5.2	54.50	266—5.2	56.97
319—8.7	55.62	319—8.7	55.51
373—0.1	56.52	351—8.4	55.96
426—3.7	57.57	373—0.1	56.52
479—7.1	58.38	394—3.9	56.75
532—10.6	59.45	426—3.7	57.31
586—2.	60.41	458—3.4	57.87
639—5.5	61.36	479—7.1	58.32
692—8.9	62.37	532—10.6	59.56
724—8.6	63.05	586—2.	60.45
		638—4.6	61.47
		692—8.9	62.59

In the first series the thermometer remained in each station so long as was found necessary by experience for the exact observation of the temperature.

The second series was undertaken in order to ascertain whether, by sinking the thermometer more or less rapidly, any difference in the results would ensue from the heat caused by he friction of the metallic cylinder against the sides of the well. However the presence of the water would lead us to expect a negative answer to this question.

From the above observations we find that from 106—6.9 feet (to which depth the temperature remains constant at 51°.68) to 724—8.6 feet deep, the temperature increases in direct proportion to the depth, and that the increase of depth corresponding to each additional degree of Reaumur is 122—2.8 feet. There is no doubt that this regularity in the increase of temperature is owing to the place of observation being perfectly free from all causes of disturbance. As former observations in other places have generally shewn great irregularities in the increase of temperature, it is probable that they have been influenced by accidental circumstances.

The lowest degree observed, 50°.9, still exceeds by 1°.35. the mean temperature of *Geneva* (49°.55). De la Rive and Marcet are of opinion that they would have found this last in a depth of 42—7.6 to 53—3.5 feet, if they had been able to penetrate into the rock itself. They think that the higher temperature of the water in the upper part of the well must be ascribed to currents of water which brought up heat from below, and which seemed probable from the clearness of the above. The influence can, however, only have extended to the first 106—6.9 feet.

CHAP. XX.—*Can the Progression of the Increase of Temperature towards the centre of the Earth be with certainty deduced from the Temperature of Artesian Springs, and the depth from which they rise?*

Arago long ago declared that artesian wells were peculiarly adapted to the determination of the rate of increase of temperature in the earth. More lately Spasky * and Kupffer † have endeavoured to calculate that rate from existing observations.

In order that such a calculation should yield accurate results, it is necessary that the following conditions should take place: —1st, That the spring should not rise from below the place of its first appearance in the well; and, 2d, That it should also have exactly the temperature of the lowest point of the well.

If the well have been sunk to within a short distance of an impervious stratum, the first condition will easily be fulfilled. But if it happen to have encountered a spring, coming from below, under a pressure sufficient to force it to the surface, the temperature of the well cannot be the same as that of the rock at the bottom. And even if the lowest point of the course of such a spring were exactly on the same level as the bottom of the well, yet it could only assume the temperature of the surrounding rock, in case it should have remained long in contact with it. This may, with probability, be supposed to be the case, when the temperature of the spring is constant. However, it may also take place, even though its temperature be variable, if its variations follow late after those of the air. So that the later in the year a spring reaches its maximum temperature, the more likely it is to shew the true temperature of the lowest point of its course.

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* Poggend. Annal. vol. xxxi. p. 365.

† Ibid. vol. xxxii. p. 284.

On the contrary, springs whose variations of temperature take place nearly or exactly at the same time as those of the atmosphere, will not give the real temperature of the lowest point of their course. Thus, the salt-springs of *Werl* can hardly be expected to shew the real temperature of the lowest point of their course, as their variations of temperature correspond almost exactly with those of the air.[*]

Spasky and Kupffer calculated, from the observations on the temperature of the artesian wells at *Vienna*,[†] an increase of temperature of 1° R. or 2°.25 in 88—5.5 and 83—1.5 feet. Kupffer found the same at 80—11.9 and 74.7.2 feet from those of *Rochelle* and *Epinay*.[‡] From Mr Griffith's[§] observations on the temperature of two springs encountered at different depths, it was calculated at 49.0.3 feet.

So rapid an increase of temperature gives room for the conjecture, that the depths of all these wells are not equal to the lowest point of the course of the springs, but that the springs rise from a still greater depth.

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[*] These thermal springs indicate the immediate influence of the external temperature, which is certainly not communicated through the earth. Their yearly mean temperature, however, coincides nearly with the temperature of the earth at the lowest point of their course. In so far it might offer a numerical element for the calculation of the increase of temperature with the depth, provided the well were as deep as the lowest point of the spring's course.

[†] Baumgärtner Journ. fur Phys. und Mathem., vol. viii. No. 3.

[‡] Journ. de Geologie. 1830. Vol. i. p. 89.

[§] Ibidem, p. 87.

From the observations of Magnus [*] and Erman, [†] in the sounding at *Rüdersdorf,* the same depth is found to be from 107.7.6 to 115—1.2 feet, and from more recent observations [‡] 121—3.8 feet.

Taking the observations at *Geneva,* cited in the last chapter, which were made under the most favourable circumstances, as the standard, the following are found to accord the most nearly with them :—

Geneva,	122 — 2.8	Feet.
Erzgebirge, from observations in enclosed waters,	136 — 11.3	...
Monk-Wearmouth, from salt-water springs,	133 — 6.8	...
Cornwall, from springs,	122 — 6.7	...
Cornwall, from enclosed waters,	118 — 3.5	...
Rüdersdorf, from rising springs,	121 — 6.9	...
Uralian Mountains, from a comparison of the most authentic observations in those regions, [§]	123 — 7.5	...

A comparison of these results leads to some interesting conclusions.

The near coincidence of these results is certainly not accidental. It rather shews that the circumstance which influences the increase of temperature towards the centre of the earth, namely the configuration of its surface, must be nearly equal in all those places. The greatest difference is between *Geneva* and the *Erzgebirge.* In Chap. XVII, I have already discussed the question, whether the value of n is the same, all other circumstances being the same, under a plain, under deep valleys surrounded by high hills, and under mountains. Should differences be found, they must be the greater between the two latter cases, which are the extremes in the configuration of the earth. The position of *Geneva* may be considered as the one extreme, and the *Erzgebirge,* in general, as the other. But whether their difference is really owing to that cause, or rather to the circumstance that the *Erzgebirge* is more exposed to the influence of the atmosphere, on account of the extent of the mines, we will leave undecided. However, the result obtained at *Monk-Wearmouth,* which so nearly accords with the value found in the *Erzgebirge,* speaks in favour of the latter supposition. The position of this shaft is such, that the circumstances must be supposed rather to resemble those at *Geneva* and *Rüdersdorf*, than those in the *Erzgebirge*; but they in so far resemble those of the *Erzgebirge*, that the influence of the atmosphere, as Phillips remarks, can only cause a depression, but not an elevation of temperature.

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[*] Poggend. Annal. vol. xxii. p. 146.

[†] Abhandlung. der Akad. der Wissenschaften zu Berlin. 1831. P. 268.

[‡] Poggend. Annal. vol. xxviii. p. 233.

[§] Erman junr. in Kämtz Lehrb. der Meteorologie, vol. ii. p. 578.

However improbable, therefore, it may be, that perceptibly different values should be found for *n* beneath plains and in mountains, it would yet be desirable that such favourable circumstances as those at *Geneva*, in the *Erzgebirge*, and in the *English mines*, should be more frequently met with and taken advantage of. Then it would not be long before a general law for the increase of temperature towards the centre of the earth would be found. And if this law should suffer interruptions, as all other physical laws do, yet a numerical expression for such interruptions might possibly be discovered.

The following Table is illustrative of the observations in Chapter VI. of this article, at p. 362 of the 20th volume of this Journal.

TABLE—*Temperatures of Springs.*

	1	2	3	4	5	6	7	8	9	10	11
	Ohskälla in Warmeland.	Lassbybackar, near Upsala.	Ditto.	Kallspring, near Lassbybackar.	Mühlspring, near Upsala.	Brennerei Spring, near Upsala.	Ditto.	Ditto.	Professors Spring, near Haga.	Ditto.	Ditto.
Years.		1809 1810	1810 1811	1809	1809 1810	1808 1809	1809 1810		1809	1810	1810 1811
January,	°	36°.86	36°.05	°	43°.79	44°.06	43°.88	°	°	41°.41	39°.83
February,		35.91			43.79	44.24	43.79			40.39	38.56
March, .		34.88	34.25		43.79	43.88	43.79	43.70		39.72	38.48
					(1811)						
April, .		34.15	33.84		43.60		43.52			38.52	39.97
May, .		35.08	33.84	44.05	43.70	44.06	43.61	43.70		38.68	
June, .		42.66				43.97				40.36	
July, .	41.18					44.06					
August, .	41.00	54.50		54.68	44.05	44.24	43.70			46.04	
September,	41.00	52.51		58.84		44.24	43.88		48.65	46.40	
October, .		46.80		50.78		44.06	43.88		47.6	46.37	
November		42.14	40.77		43.88	44.06	43.88		44.64		44.96
December,		37.98	37.94			44.24	43.79		42.35		42.26
Mean, .	41.06	41.71			43.85	44.098	43.77	43.70	42.26	41.99	
Differ. between Max. and Min.	0.18	20.34			0.45	0.36	0.36		6.3	7.85	

TABLE—continued.

	12	13	14	15	16	17	18	19	20	21	22
	Near Haga.	Priester Spring, near Haga.	Gnesta Windmühle, on the road to Fundbo.	Norbygard in Bondkyrka John.	Norbylund.	Mysbygard in Danmarkssocken.	Staby Spring in Naessocken.	In the neighbourhood of Colinton, near Edinburgh.	Ditto.	Konigsberg in Prussia.	Louisen Spring near Berlin.
Years.	1809 1810	1809 1810	1810 1811	1809 1810	1810 1811	1810		1827 1828	1827 1828		1811 1812
January,	43°.52	°	°	42°.98	°	°	°	50°.85	49°.19	45°.02	49°.45
February,	43.34			42.8				49.30	46.06	44.19	49.10
March, .			42.26	42.8				49.88	48.67	43.74	49.10
		(1811)		(1811)							
April, .	42.08	43.34	42.26	42.8	42.26			49.34	49.34	44.73	48.91
May, . .	42.08	43.70	42.53	42.8	42.8	43.15		50.31	51.89	45.18	
June, . .				42.8			43.57	52.54	55.19	46.58*	
July, . .								53.55	56.46	48.01*	
August, .	43.15	43.88	42.8	42.98	43.60	43.15		53.05	55.75	49.23*	49.45
September,	43.88	{43.88 44.24	43.15	43.34		43.34	43.70	52.32	54.81	49.77*	49.45
October,		44.05	43.43			43.34		52.38	53.71	49.41	49.45
November,		43.92						51.75	50.13	47.97	
December,	43.52							51.41	49.30	46.53	49.45
Mean, .	42.98	43.96	42.98	42.93	43.20	43.25		51.39	51.70	46.85	49.30
Differ. between Max. and Min.	1.8	0.9	1.17	0.54				4.25	10.39	6.03	0.54

	23	24	25	26	27	28	29	30	31	32	33
								Mineral Springs at Meinberg.			
	On Ravensbergen, between Bergholz and Langerwisch.	Teltower Vorstadt, near Potsdam.	Ditto.	Near the Havel, between Potsdam and Templin.	Ditto.	Templin, near Potsdam.	Ditto.	Trinkquelle.	Neubrunnen.	In Stern.	Schwefelquelle.
Years.		1814 1819	1819	1817 1819	1819	1814 1819	1819	1830 1831	1830 1832	1830 1832	1830 1832
January,	50°.00	44°.57	44°.05	49°.48	49°.41	47°.84	47°.48	°	°	°	°
February,	50.00	45.09	44.97	49.48	49.48	48.88	47.88	44.37	44.37	41.56	38.32
March, .	50.00	46.28	46.28	49.45	49.32	48.42	48.35	44.52	44.37	42.12	
April, .	50.09	47.88	47.88	50.11	50.22	48.65	48.53	45.50	44.37	42.12	38.75
May, .	50.00	51.73	51.98	50.31	50.45	49.66	49.55	46,62	46.62		48.87
June, .	50.09	53.78	55.06	50.67	50.78	50.58	50.56				
July, .	50.09	55.53	57.46	50.90	50.90	50.56	50.56				
August, .	50.22	56.07	57.42	51.23	51.68	50.78	51.10	54.27	55.96	53.37	61.25
September,		53.43	53.43	51.12	51.12	50.56	50.56				
October,	50.18	52.25	50.9	50.26	50.00	49.73	49.88				
November,	50.09	48.57	48.08	49.45	49.36	49.34	49.32				
December,	50.00	45.18	43.92	49.45	49.43	48.17	48.20				
Mean, .	50.07	50.22	50.11	50.16*	50.18	49.34*	49.32	47.41	47.54	44.15	44.01
Differ. between Max. and Min.	0.225	11.49	13.54	1.77	2.27	2.94	3.62	10.12	11.58	11.81	22.92

TABLE—continued.

	34	35	36	37	38	39	40	41	42	43	44
	Salt Springs at Werl.										
	Michael's.	Shaft.	Bore.	Holes.	Maximilian.	Shaft.	Well in Düsseldorf.	Queckbrunnen, near Burgbrohl.	Ditto.	Mineral Spring near Burgbrohl, No. 1.	Ditto.
Years.	1832	1833	1832	1833	1832	1833	1831	1833	1834	1833	1834
	°	°	°	°	°	°	°	°	°	°	°
January,		44.66		49.28		54.20	47.52		(1835) 49.10		
February,		47.75		50.00		53.03	47.30		(1835) 48.42		
March, .	46.03	47.22	50.49	50.18	53.08	52.79	47.86		48.65		52.92
April, .	51.46	48.85	52.65	52.10	54.74	55.37	47.88	45.95	46.62	52.70	53.15
May, .	53.26	51.21	53.17	55.94	53.82	56.32	48.06	46.85	46.85	52.92	
June, .	52.90	52.92	57.45	55.51	54.70	55.51	49.75	47.75	47.52	53.37	53.37
July, .	53.64	53.91	54.85	55.92	55.21	57.31	50.63	48.46	48.65	53.60	53.60
August, .	53.60	53.62	54.87	54.57	55.17	56.75	51.42	48.87	49.10	53.37	53.60
September,	53.15	52.70	54.59	55.40	54.83	55.85	52.83	49.32	50.00	53.48	
October,	51.44	52.51	52.70	54.95	55.44	52.62	53.33	49.70	50.90	53.37	
November,	48.87	51.12	50.42	53.95	50.06	49.66	50.99		50.45		
December,	46.26	48.20	50.13	53.10	52.25	52.51	48.42	48.87	49.77	53.15	
Mean, .	51.15	50.38	52.60	53.48	53.71	54.32	49.66	48.22	48.65	53.24	53.37
Differ. between Max. and Min.	7.15	9.24	4.74	6.63	2.96	4.52	6.03	3.80	4.50	0.9	0.675

	45	46	47	48	49	50	51	52	53	54	55
							Fresh Springs at Basel.				
	Mineral Spring near Burgbrohl, No. 2.	Ditto.	Mineral Spring near Burgbrohl, No. 3.	Ditto.	Mineral Spring at Tönnistein.	Hottest spring at Burtscheid.	Lochbrunnen at the Herbrigberg.	Lochbrunnen, near the Post-office.	Well behind the School.	Corn-market Well.	Gerber Well.
Years.	1833	1834	1833	1834	1835	1833 1834	1821	1821	1821	1821	1821
	°	°	°	°	°						
January,						159°.35	44°.95	47°.65	48°.01	47°.65	49°.10
February,						159.8	42.8	46.40	47.11	46.75	49.01
March, .		57.65		56.30		160.25	42.8	47.11	47.48	47.30	48.74
April, .	57.87	57.31				160.13	43.51	48.01	48.20	48.20	48.74
May, .	57.87		57.76		55.40	160.70	48.65	48.91	48.38	48.38	48.74
June, .	58.10	58.10		57.65		160.70	47.48	49.45	48.74	48.91	48.74
July, .	58.10	58.10	57.98	60.35	55.40		48.74	49.81	49.28	49.45	49.01
August, .	58.10	58.21	57.65		55.17	159.57	50.18	50.18	49.68	49.81	49.01
September,	58.10	58.21	67.53	57.42	55.17	158.22	52.51	51.98	51.8	51.44	50.18
October,	58.10		57.20		54.95	158.22	52.34	50.90	50.9	50.90	50.00
November,						158.67	50.00	49.45	49.68	59.64	49.64
December,	58.10					158.9	48.55	49.45	49.45	49.45	49.64
Mean, .	58.05	57.91	57.63	57.91	55.21	159.51	47.52	49.12	49.05	58.95	49.19
Differ. between Max. and Min.	0.225	0.90	0.79	4.05	0.45	2.47	9.71	5.58	4.68	4.68	1.36

TABLE—continued.

	56	57	58	59	60	61	62	63	64	65
	Fresh Springs at Basel.			Stuttgart.						
	Blömli's Well.	St Alban Thal Well.	Well on the Münsterplatz.	Temperature of the spring.	Temperature of the air.	Temperature of the spring.	Temperature of the air.	Temperature of the spring.	Temperature of the air.	Temperature of the spring.
Years.	1821	1821	1821	1828	1829	1829	1829	1830	1830	1831
January,	47°.65	50°.00	46°.40	43°.02	37°.17	38°.84	24°.69	34°.77	15°.8	37°.98
February,	46.94	50.00	44.60	42.25	35.24	36.36	28.72	34.77	27.89	37.47
March, .	46.04	49.45	46.21	43.74	42.25	38.67	39.62	37.75	44.57	42.16
April, .	46.75	49.81	47.84	47.97	50.76	45.95	50.58	47.30	54.18	48.15
May, .	48.20	49.28	49.64	53.64	60.39	52.37	58.12	53.60	60.72	52.40
June, .	49.45	49.81	50.35	58.47	65.72	57.35	62.09	56.77	62.71	56.50
July, .	50.00	49.64	52.51	62.09	68.9	61.38	67.84	60.30	69.19	61.02
August, .	51.43	50.00	53.60	60.39	63.27	61.47	63.25	61.43	65.79	61.67
September,	53.60	51.08	54.31	59.04	59.22	58.17	55.73	57.29	55.89	57.76
October,	53.24	51.25	52.88	54.52	48.83	52.45	46.37	53.48	48.10	55.98
November,	51.43	50.90	49.28	47.00	40.90	44.73	34.76	48.06	43.27	48.83
December,	50.00	50.58	48.20	42.91	37.44	38.70	21.52	41.22	33.14	44.10
Mean, .	49.57	50.13	49.66	49.10	51.68	48.87	46.73	51.44	48.42	50.34
Differ. between Max. and Min.	7.53	1.8	9.72	19.93	33.66	25.11	43.15	26.95	53.39	24.21

	66	67	68	69	70	71	72	73	74	75
	Stuttgart.							Tubingen, temperature of the spring	Ditto.	Ditto.
	Temperature of the air.	Temperature of the spring.	Temperature of the air.	Temperature of the spring.	Temperature of the air.	Monthly mean temperature of this spring.	Monthly mean temperature of the air.			
Years.	1831	1832	1832	1833	1833			1831	1832	1832
January,	26°.7	39°.32	30°.43	36°.05	24°.44	38°.39	26°.85	39°.87	42°.68	35°.52
February,	38.50	39.42	33.95	36.68	35.71	37.78	33.22	39.87	41.56	38.88
March, .	44.71	40.55	39.49	39.58	37.58	40.41	41.56	43.51	43.38	43.11
April, .	52.56	47.02	49.84	43.94	46.60	46.62	50.79	48.33	48.01	46.26
May, .	58.45	51.75	56.61	54.68	66.08	53.05	56.34	52.51	52.38	53.07
June, .	63.27	57.20	62.24	60.08	68.20	57.72	64.06	56.41	56.25	59.13
July, .	68.00	61.67	66.10	60.93	63.54	61.25	67.30	58.21	59.26	56.32
August, .	65.56	63.38	67.47	58.93	59.47	61.20	64.13	60.12	61.38	55.49
September,	56.52	58.55	56.75	55.51	56.07	57.65	56.70	56.75	56.61	55.32
October,	56.67	53.90	48.35	52.49	47.79	53.80	49.34	55.06	50.40	52.08
November,	41.58	46.02	38.07	46.80	40.95	46.98	39.91	49.70	44.24	49.01
December,	47.64	40.02	34.92	44.28	41.97	41.9	34.43	45.36	40.28	46.09
Mean, .	51.01	48.60	48.85	49.23	49.14	49.73	49.30	50.47	49.68	49.23
Differ. between Max. and Min.	39.44	23.96	37.05	24.88	43.76	23.47	40.45	20.25	19.82	23.60

No. [illegible]. Wahlenberg, Gilbert's Annal. v. x[illegible] p. 113. and fol.

No. [illegible] and [illegible]. Bergman's Hertha, Jan. [illegible], v. xiii. p. 20. Spring 19 rises out of a bed of gypsum, and is situated [illegible] feet above the level of the sea. No. 20, which rises out of clay, lies [illegible] feet above the sea.

No. [illegible]. Erman jun. Poggend. Ann. v. [illegible] p. [illegible] and fol. The temperatures marked * are found by interpolation.

No. [illegible]. Wahlenberg, ibid. p. [illegible]. Erman sen. (see Abhandlungen der Kön. Akad. d. W. zu Berlin für [illegible] p. [illegible] and fol.) observes that Wahlenberg's scale of variations in the temperature of this spring remained the same during several years. According to him its mean is 45°.66.

No. [illegible]. Erman sen. as above.

* No. [illegible]. Brandes die Mineralquellen &c. zu Meinberg, Lemgo 1832, p. 271, [illegible], [illegible], and [illegible].

No. [illegible]. communicated by M. v. Lilien, Director of the Salt Works at Werl.

No. [illegible]. communicated by Prof. Benzenberg in Dusseldorf.

No. [illegible]. Observ. of the author.

No. [illegible]. communicated by M. Wagner, Surveyor of Aix-la-Chapelle.

No. [illegible]. Abhandlung über die Wärme der Erde in Basel, von Merian. Basel, [illegible].

No. [illegible]. According to the communication of Prof. Plieninger in Stutgardt.*

Remarks on the Geographical Position of some Points on the West Coast of Scotland. By WILLIAM GALBRAITH, M.A., Teacher of Mathematics, Edinburgh, M. S. A. Communicated by the Society of Arts.[†]

HAVING for some years past been in the habit of visiting several points on the west coast of Scotland, I employed my leisure hours occasionally in determining their latitudes and longitudes, and was often forcibly struck with the wide deviation from the truth which many of them exhibited.

It is true, I was told when I made a few remarks on this subject before the Society of Arts last year, that errors were known to exist in our best maps and charts, and that the late Dr MacCulloch advanced their imperfections as a reason for delaying the publication of his researches, said to be intended for the basis of a geological survey of Scotland. It is indeed easy to charge most maps with errors, but it is not quite so easy to shew their extent and to correct them; at least I am not aware that Dr MacCulloch did this in any great degree, which, from the time he might have employed, by means of the funds placed at his disposal (had he used them as he ought to have done) he might have effected.

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* For the reductions of the degrees to the scale of Fahrenheit in the Memoir of Bischof, I am indebted to Mr George Atkin, author of the useful Thermometrical and Barometrical Tables, published by Messrs A. & C. Black, Edinburgh.

[†] Read before the Society of Arts, January 10. 1838.

Having shewn, last year, that some of those reckoned our best maps, such as Arrowsmith's, Wyld's, &c., contained very considerable errors in the Firth of Clyde, especially about the Island of Arran, I was in hopes that, in calling attention to the subject through this Society, by definite and palpable instances, the claims of Scotland could not well be overlooked by the Legislature, especially if followed up by persons of influence connected with the northern part of the island. In this I have not been disappointed. A few months after my remarks were read in the Wernerian Society, and published in Professor Jameson's Journal, the Wernerian Society memorialised Government, and also recommended the subject to the consideration of the Royal Society of Edinburgh, the Highland Society, and the Magistrates of Edinburgh, and they also petitioned Government to order the resumption of the *Trigonometrical Survey of Scotland.*

I have now the satisfaction of informing this Society, on the best authority, received some months ago, as may have already been learned by other means, that the Triangulation of the West Coast of Scotland, under the able superintendence of Colonel Colby, will be resumed next summer.

In addition to this, as a piece of scientific intelligence, I may add, that the original observations on the fixed stars, made with the Ordnance zenith sector, are also now in the course of reduction. Preparatory to this, that instrument has been employed for some time past at Greenwich, in order to compare observations made by it, with those made by the mural circles, to test its accuracy, so that no reasonable doubt can now be cast on its final results.

When these and an arc of the meridian, traversing Britain from perhaps the Isle of Wight on the south, to the island of Balta in Shetland on the north, are completed, we shall then have an arc of about 10° of the meridian measured with extreme precision in Europe, to be compared with another in British India of still greater extent, and both executed with British instruments of great accuracy, by British observers of undoubted talents, and consequently the final results will possess all the refinements which modern science can give. From these we will then be enabled to deduce the true figure and magnitude of the earth from British measures alone. Men of science will hail the completion of these labours with great satisfaction, while in the accurate survey and delineation of the natural features of our own country, its commercial, manufacturing, and agricultural interests will be greatly improved.

Indeed the Coast Survey of the Firth of Clyde is now in progress by Captain A. Henderson of the Royal Engineers, who is provided with excellent instruments, made by Messrs Troughton and Simms, and he possesses all the requisite talents, perseverance, and industry for such a duty in an eminent degree. He has already fixed nearly all the more conspicuous and important points, to serve as the fundamental basis whereon the marine surveyors must form their charts, which, when finished, cannot fail to benefit greatly the shipping trade of the Clyde. This gentleman's labours would, perhaps, in the mean time, have terminated about Arran, had not the Chamber of Commerce of Greenock, and others interested in the prosperity of the trade of the Clyde, been informed of this last year, and consequently very properly urged the continuation of his valuable labours up as high as Greenock and Port-Glasgow, which are now in progress there. Under these circumstances, perhaps, the results which I am now able to communicate cannot possess any great and permanent value, as they will eventually be superseded by those more accurate determinations to which I have now alluded. However, as the latter will not be generally communicated to the public for some time, perhaps I may be excused for troubling the Society with mine.

The instruments which I generally carry with me for this purpose, are a small altitude and azimuth astronomical circle, and a good chronometer, when I can fortunately fall in with one. For this I have been indebted to Mr Bryson, in the present as well as last year. The circle has three verniers, each reading to 10″, and a good level, each division of which shews 3″, though 1″ may be estimated, if thought necessary. The verniers are read by compound lenses, on the tubes of which are placed reflectors supplied by Messrs Adie and Son, that greatly facilitate the reading, especially at night, in observations upon the stars. Such an instrument, when properly used, is capable of giving good results either with regard to latitude or time.

I had also a small reflecting box-circle made by Messrs Adie, which I had the pleasure of laying before this Society some time ago, and which, on several occasions, I found very serviceable, both on account of its accuracy and portability. Indeed, when the arcs are read alternately on both sides of zero from three verniers, I think it may be depended upon, both for latitude and time, to a very considerable degree of accuracy, especially when the repetitions are continued for a considerable time. I should say that, by circum-meridian observations with an artificial horizon, near noon or the meridian, for about ten minutes before and ten minutes after transit, the results, when properly reduced, could hardly, under favourable circumstances, differ more than 20″ or 30″ from the truth, and frequently much less, though such a degree of accuracy could not generally be expected. With regard to time, I generally found it to give results not more than two or three seconds from the truth, though it would require more observations than I have yet had an opportunity of making, to test its accuracy completely.

Of course, Kater's circle being pretty large and well divided, its results will seldom differ more than 10″ of arc from the truth, when used with care in favourable circumstances and good weather.

The chronometer performed remarkably well, and what was not a little singular, the rate continued sensibly the same throughout the whole time of my absence, or at least the rate was the same when I returned to Edinburgh, as when I left it; and I deduced almost the same rate from an interval of about ten days at one place, where I remained stationary for that time.

At Edinburgh, on the 3d of August 1837, at noon, the chronometer was fast of mean time, ... $0^h\ 0^m\ 4^s.5$
Daily rate *losing*, ... $0\ \ 0\ \ 1.28$

From these data the following table was formed, for conveniently making the necessary comparisons.

Table of Error of the Chronometer.

		s.			s.
August 4.	fast	+ 3.22	August 20.	slow	— 17.26
... 5.	...	+ 1.94	... 21.	...	— 18.54
... 6.	...	+ 0.66	... 22.	...	— 19.82
... 7.	slow	— 0.62	... 23.	...	— 21.10
... 8.	...	— 1.90	... 24.	...	— 22.38
... 9.	...	— 3.18	... 25.	...	— 23.66
... 10.	...	— 4.46	... 26.	...	— 24.94
... 11.	...	— 5.74	... 27.	...	— 26.22
... 12.	...	— 7.02	... 28.	...	— 27.50
... 13.	...	— 8.30	... 29.	...	— 28.78
... 14.	...	— 9.58	... 30.	...	— 30.06
... 15.	...	— 10.86	... 31.	...	— 31.34
... 16.	...	— 12.14	September 1.	...	— 32.62
... 17.	...	— 13.42	... 2.	...	— 33.90
... 18.	...	— 14.10	... 3.	...	— 35.18
... 19.	...	— 15.98	... 4.	...	— 36.46

With the assistance of this table, the calculations for latitude and longitude were facilitated.

1. The Latitude of the Roman Catholic Chapel at Ayr was determined to be,

by observations on August 8, Sun, ...	55° 28′ 58″ N.
... 8, Stars, . . .	54
... 24, Stars, . . .	50
... 24, Stars, . . .	49
... 24, Polaris, . . .	55
Mean latitude, . . .	55 28 53.2N.

2. Longitude by Chronometer:—

	h. m. s.
August 24, ...	0 18 27.0 W.
... 26, ...	26.7
... 29, ...	25.8
... 30, ...	23.7
... 31, ...	23.8
Mean longitude in time, . .	0 18 25.4 W.
... ... in degrees, . .	4° 36′ 21″ W.

3. In like manner, I made the position of Portincross Castle to be, from observations taken

August 9, Latitude by Sun, .	55° 42′ 9″ N.
... 11, Polaris, . .	13
... 11, α Aquilæ, . .	11
... 12, Sun, . .	7
Mean latitude, ...	55 42 10 N

		h. m. s.
4. August 11.	Longitude by chronometer,	0 19 36.0 W.
... 11.	... ...	34.0
... 19.	... ...	37.0
... 19.	... ...	35.0
	Mean longitude in time,	0 19 35.5 W.
	... ... in degrees,	4° 53′ 52.5″W

The observations from which these results have been obtained were not so numerous as I could have wished, because part of my time was employed in other pursuits; but if compared with many of our books and charts, they will be found, I believe, more accurate. In fact, all the coast, from about the Cloch Light to the Mull of Galloway, appears, by my own observations, combined with those of others, to be placed too far eastward by several miles. For example, the position of Ayr Light is given by different authors as follows:—

5. The latitude is given—

By Mackay's Navigation,	1804 and 1809, at	55° 25′ 0″ N.
... Lynn's Nautical Tables,	1825,	25 0
... Norie's Navigation,	1828,	25 0
... ...	1835,	28 30
While I found my station in 1837,		28 53

nearly the same as last, though the *light* appears to be really a few seconds to the north of my position.

6. Again, the longitude by

Mackay in 1804,	4° 26′ 0″ W.
Lynn in 1825,	26 0
Norie in 1828,	26 0
... ... 1835,	37 0
The preceding, from my observations, is	4 36 21 W.

nearly agreeing with the last of Norie's, though the *light* may indeed be about half a minute to the westward of my position.

I think it would have been credited by few, that the geographical position of the county town of Ayr had been till within these few years so badly determined, or at least so erroneously given in our best books and charts, on which it was supposed implicit confidence might have been placed.*

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* Dr Mackay states, in the explanation of his Table of Latitudes and Longitudes, that "the positions of many of those places on the west coast of Scotland were communicated to him by Mr Lamont of Greenock." I have been

Indeed it never could have occurred to me to suspect, that the positions of such important sea-ports as that of Ayr, and others adjacent, were erroneous to the extent of 2′ or 3′ of latitude, and 8′ or 10′ of longitude, had I not been led to examine the subject somewhat attentively, from having noticed similar errors in Arran last year, where, in the maps of Arrowsmith, Wyld, &c., the whole southern shore of Arran is placed about 5′ north of its true position, and it is not improbable that the whole island is.

These instances justify the call that has been made to resume the Trigonometrical Survey of Scotland, and there is now every prospect that it will be duly attended to, so that the positions of different important points in this part of the island, as well as many others elsewhere which I have not had an opportunity of examining, though equally or more erroneous, will be speedily corrected. Chiefly, I believe, to the exertions of the more influential members of several societies belonging to Scotland, the *immediate* resumption of the Trigonometrical Survey is owing, though others certainly had previously lent their powerful aid in the application.

In the report to the British Association which met at Liverpool in September 1837, it is stated that "the Association made application soon after their meeting at Edinburgh" (in 1834) "for the resumption of the Trigonometrical Survey of Scotland,—a work imperiously called for by the imperfect state of our best maps and charts of that part of the island, either for the purposes of geology or navigation. It is needless to give farther proof than that parts of several of the *large islands at the mouth of the Clyde* are laid down several miles out of their true position. The magnificent scale on which the Survey of Ireland is now carrying on, emboldened many scientific societies of Scotland this year (1837) to memorialise government on the subject." The report goes on to say, "I am happy to add that the applications have been successful, and the triangulation of Scotland will commence early in 1838." It there-

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informed that Mr L. has been forming a chart of those coasts, into which he has introduced many corrections, and that, it is to be regretted, has never been published.

fore appears that till the application of the scientific societies of Scotland in the summer of 1837, the application of the British Association for at least the *immediate* resumption of the Survey had been unsuccessful. The benefits, however, that will now be conferred on the country must be great and permanent. The speculations of geologists will likely be more consistent with regard to localities, dips, and directions of mountain-ranges, the views of the agriculturist, as connected with drainage, &c., better founded, and the prospects of the mercantile interest more secure.

Observations on the Hurricanes and Storms of the West Indies and the Coast of the United States. By W. C. Redfield, Esq. With a Chart.*

From a careful attention to the progress and phenomena of some of the more violent storms which have visited the Western Atlantic, I have found that they exhibit certain characteristics of great uniformity. This appears, not only in the determinate course which these storms are found to pursue, but in the direction of wind, and succession of changes which they exhibit while they continue in action. The same general characteristics appear also to pertain, in some degree, to many of the more common variations and vicissitudes of winds and weather, at least in the temperate latitudes. The following points I consider as established:

1. The storms of greatest severity often originate in the tropical latitudes, and, not unfrequently, to the eastward of the West India Islands; in the tropical regions they are distinguished by the name of *hurricanes.*

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* In the 18th Volume of this Journal we published a short account of Mr Redfield's "Observations on the Hurricanes and Storms of the West Indies and the Coast of the United States," being convinced his statements were deserving the particular attention of Meteorologists and Mariners. It is but lately, however, that his views have been particularly noticed in this country. Mrs Somerville, Professor Daubeny, and other writers, have mentioned them with approbation, and therefore, we presume, we are consulting the wishes of our readers by laying before them this more extended memoir of Mr Redfield.

2. These storms cover, at the same moment of time, an extent of contiguous surface, the diameter of which may vary in different storms, from one hundred to five hundred miles, and in some cases they have been much more extensive. They act with diminished violence towards the exterior, and with increased energy towards the interior, of the space which they occupy.

3. While in the tropical latitudes, or south of the parallel 30°, these storms pursue their course, or are *drifted* by the natural atmospheric current of the region towards the west, on a track which inclines gradually to the northward, till it approaches the latitude of 30°. In the vicinity of this parallel, their course is changed somewhat abruptly to the northward and eastward, and the track continues to incline gradually to the east, towards which point, after leaving the lower latitudes, they are found to progress with an accelerated velocity.

The rate at which these storms are found thus to advance in their course, varies in different cases, but may be estimated at from 12 to 30 miles an hour. The extent to which their course is finally pursued, remains unknown; but it is probable that as they proceed, they become gradually extended in their dimensions, and weakened in their action, till they cease to command any peculiar notice. One of the hurricanes of August 1830, has been traced in its daily course, from near the Caribbee Islands to the coast of Florida and the Carolinas, and from thence to the banks of Newfoundland, a distance of more than three thousand miles, which was passed over by the storm in about six days. The duration of the most violent portion of this gale, at the different points over which it passed, was about 12 hours, but its entire duration was in many places more than twice that period. Another hurricane which occurred in the same month, passed from near the Windward Islands, on a more eastern but similar route, and has also been traced in its daily stages by means of the journals and reports of voyagers, near two thousand five hundred miles. It was in this storm that the Russian Corvette, Kensington, Captain Ramsey, suffered so severely. The hurricane of August 1831 which desolated the island of Barbadoes on the 10th of that month, the daily progress of which has also been ascertained, passed in nearly a direct course to the northern shores of the Gulf of Mexico and New Orleans, where it arrived on the 16th of the same month, having passed over a distance of twenty-three hundred statute miles in six days after leaving Barbadoes.* Many cases of like character might be adduced.

4. The duration of the storm, at any place within its track, depends upon its extent and the rate of its progressive velocity, as these circumstances are found to determine the time which is required for the storm to pass over any given locality falling within its route. Storms of smaller extent, or dimensions, are usually found to move from one place to another with greater rapidity than larger storms.

5. The course thus pursued by the storm, is found to be entirely independent of the direction of wind which it may exhibit at the different points over which it passes—the wind in all such storms being found to blow after the manner of a whirlwind, around a common centre or vortex, during their entire progress, and in a determinate direction or course of rotation, which is from *right* to *left* (or in the direction from west to south) horizontally. The direction of the wind, therefore, for the most part, does not coincide with the course of the storm.

6. In the lower latitudes while drifting to the westward, the direction of the wind at the commencement, or under the most advanced portion of these storms, is from a *northern* quarter, usually at some point from north-east to north-west, and durring the latter part of the gale, it blows from a southern quarter of the horizon, at all places where the whole effect of the gale is experienced.

7. After reaching the more northern latitudes, and while pursuing their course to the northward and eastward, these storms commence with the wind from an eastern or southern quarter, and terminate with the wind from a western quarter, as will appear more distinctly under the three following heads, the latter portion of the storm being usually attended with broken or clear weather.

8. On the outer portion of the track, north of the parallel of 30°, or within that portion of it which lies *farthest* from the

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* The tracks of these and other hurricanes appear on the annexed chart.

American coast, these storms exhibit at their commencement a *southerly* wind, which, as the storm comes over, *veers gradually to the westward*, in which quarter it is found to terminate.

9. In the same latitudes, but along the *central portions of the track*, the first force of the wind is from a point near to *south-east*, but after blowing for a certain period, it *changes suddenly*, and, usually after a short intermission, to a point nearly or directly opposite to that from which it has previously been blowing, from which opposite quarter it blows with equal violence till the storm has passed over, or has abated. This sudden change of a south-easterly wind to an opposite direction, *does not occur towards either margin of the storm's track*, but only on its more central portion, and takes effect in regular progression along this central part of the route, from the *south-west* towards the *north-east*, in an order of time, which is exactly coincident with the progress of the storm in the same direction. It is under this portion of the storm that we notice the greatest fall of the barometer, and the mercury usually begins to rise a short time previous to the change of wind. In this part of the track, the storm is known as a *south-easter*, and is usually attended with rain previous to the change of wind, and perhaps for a short time after.

10. On that portion of the track which is *nearest* the American coast, or which is farthest inland if the storm reaches the continent, the wind commences from a more eastern or north-eastern point of the horizon, and afterwards veers more or less gradually by north, to a north-western or westerly quarter, where it finally terminates. Here also the first part of the storm is usually, but not always attended with rain, and its latter or western portion with fair weather. The first or foul weather portion of the storm, is, on this part of its track, recognised as a *north-easter*.

It should be noted, however, that near the latitude of 30°, and on the shores of Carolina, where the storm enters obliquely upon the coast, while its track is rapidly changing from a northwardly to an eastwardly direction, the wind on the central track of the storm will commence from an eastern or north-eastern point of the compass, and will gradually become south-easterly as the storm approaches its height.

11. A full and just consideration of the facts which have been stated, will shew conclusively that the portion of the atmosphere which composes for the time being the great body of the storm, whirls or blows as above stated, in a horizontal circuit, around a vertical or somewhat inclined axis of rotation which is carried onward with the storm ; that the course or direction of this circuit of rotation is from *right* to *left ;* and that the storm operates nearly in the same manner as a tornado or whirlwind of smaller dimensions; the chief difference being in the more disk-like form of the whirling body and the magnitude of the scale of operation.* This view of the subject, when fully comprehended, affords a satisfactory solution of the otherwise inexplicable phenomena of storms, and will also be found to accord entirely with the fact which appears in the above statement, that in the phases or changes which pertain to a storm, the wind, on one margin of its track, veers *with the sun*, or from *left* to *right*, while under the opposite margin of the same storm it veers *against the sun*, or from *right* to *left ;* for this peculiarity necessarily attends the progressive action of any whirlwind which operates horizontally.

12. Owing to the centrifugal action of these rotative storms, the barometer, whether in the higher or lower latitudes, always sinks while under the first portion or moiety of the storm on every part of its track, excepting, perhaps, its extreme outward margin, and commonly affords us the earliest and surest indication of the approaching tempest. The mercury in the barometer always rises again during the passage of the last portion of the gale, and commonly attains the maximum of its elevations on the entire departure of the storm.

The great value of the barometer to navigators is becoming well understood, and its practical utility might be greatly increased by hourly entries of the precise height of the mercurial column, in a table prepared for the purpose. Its movements, unless carefully recorded, often escape notice or recollection; which may easily happen at those times when a distinct knowledge of its latest variations might prove to be of the greatest importance.

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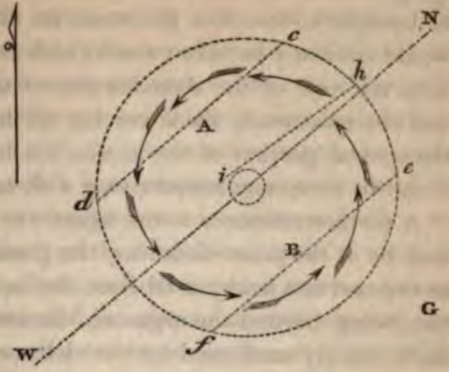
* It is to be understood that the diameter of the whirlwind which constitutes the storm is commensurate with the width of the track over which the storm passes. The main body of the storm is supposed to move in the form of an extensive disk, whirling around its own centre as it advances in its regular track—with this difference, that the rotative movement is far more rapid in the interior portions of the whirling body, than towards its exterior limits.

In the foregoing statements our design has been to designate in a summary manner the principal movements which, in these regions at least, constitute a storm; and we do not attempt to notice the various irregularities, and subordinate or incidental movements and phenomena of the atmosphere with which a storm may chance to be connected, or which may necessarily result from such violent movements in a fluid which is so tenuous and elastic in its character. It may be remarked in general, that the most active or violent storms are usually the most regular and uniform in the development of those characteristic movements which we have already described. It is also probable, that the vortex or rotative axis of a violent gale or hurricane, oscillates in its course with considerable rapidity, in a moving circuit of moderate extent, near the centre of the hurricane; and such an eccentric movement of the vortex may, for aught we know, be essential to the continued activity or force of the hurricane. Such a movement will fully account for the violent *flaws* or *gusts* of wind, and the intervening *lulls* or remissions, which are so often experienced towards the heart of a storm or hurricane, when in open sea; but of its existence we have no positive evidence.

In Purdy's Memoir of the Atlantic Ocean it is stated, "That while one vessel has been lying to in a heavy gale of wind, another, not more than thirty leagues distant, has, at the very same time, been in another gale equally heavy, and lying to with the wind in an opposite direction."

This statement is obviously to be understood as applicable to two vessels falling under the two opposite sides or portions of the same storm, where the wind in its regular circuit of rotation must, of course, blow from the opposite quarters of the horizon. We will suppose one of the vessels to be at A and the other at B, in the annexed figure. The storm in pursuing its course from W. towards N., will strike the first-mentioned vessel in the direction which is shewn by the wind-arrows at the point *c*, which, if the position be in the temperate latitudes, north of 30°, will be from eastward. Now, it is obvious, that as the storm advances in its course north-eastward, this vessel, if nearly stationary, will intersect the body of the gale on the line *c* A *d*. As the storm advances, the wind must also veer to the northward, as shewn by the arrows, being at N.E. when the vessel is brought under the point A, and near the close or departure of the storm by its further progress eastward, the wind will have further veered to the direction shewn at *d*, which, with due allowance for the progressive motion of the storm, we will set down at N.N.W. The other vessel, as is equally obvious, will first take the wind from the southward, as shown at *e*, in which quarter it will blow, with no great variation, till, by the advance of the storm, the ship is brought under the point B. The barometer, which had previously been falling, will now commence rising, and the wind, veering more westerly, will, at the departure of the storm, be found in the direction shewn at *f*, which, after the allowance already referred to, may be stated at W.N.W. Such, substantially, are the facts commonly reported by vessels which fall under the lateral portions of the Atlantic storms, and it is readily seen, that the opposite winds which are exhibited on the two different intersections of the storm, as above described, will very naturally be mistaken for two separate and distinct gales.



The phases of the wind in these gales are, however, in all cases modified more or less by the course or changing position of the vessel exposed to its action. For example, a ship on taking the gale, say at E.S.E. at the point *h* on the figure, and lying to with her head to the northward, may by that means be brought to intersect the storm on the line *hi*, at the point *i*, would suddenly be taken aback, with the wind, say at N.N.W., as in the case of the Jamaica homeward-bound fleet in 1782, and the barometer, which reaches its lowest depression under the central portion of the storm, would about this period be found to have commenced rising with some degree of rapidity.

A further reference to the figure will shew that a ship, which may be at the point G during the passage of the gale, would be exposed to a heavy swell from the southward and westward; but, being beyond the organized limits of the storm, may remain entirely unaffected by the violence of the wind, which at the same time may be raging with destructive fury at the distance of a few leagues. The writer has knowledge of many such examples.

It has been suggested that "the larboard tack is the proper one to lie to on, as the wind will then be found to draw aft;" but this will frequently prove erroneous, as the wind may draw either way, on either tack, according to the position and course of the ship, in the storm, and the extent and rate of progress of the latter. In the case of the fleet which encountered the gale of 1782, it was probably the best course to carry sail to the northward at the very commencement of the gale and as far and as long as possible. By this means the fleet might, perhaps, have been drawn as far northward as the point A on the figure, and the change of wind to the northward and westward would have been rendered more gradual. The chief difficulty and danger, is when the direction of the wind at the first setting in of the gale, is found to be nearly at right angles with the known courses of the storms in the region where the gale is encountered, and it is then desirable to pursue such a course as to avoid, if possible, falling into the heart of the storm.

It frequently happens that a storm, during the first part of its progress over a given point, fails to take effect upon the surface, while it exhibits its full activity at a greater altitude. This commonly happens when this portion of the storm arrives from, or has recently blown over, a more elevated country, or is passing or blowing from the land to the sea. On land, the most violent effects are usually felt from those storms which enter and blow from the open ocean upon the shores of an island or continent. Upon the latter, under such circumstances, the *first* part of the gale is usually the most severe, and that coast of an island upon which a storm first enters, or blows, also suffers most from the early part of the gale, but its latter, or receding part, often acts with the greatest fury upon the opposite side of the island, which had previously derived some degree of shelter from the intermediate elevations and other obstacles opposed to the force of the wind, the benefit of which is now lost by its counter direction from the open ocean. Owing to similar causes, the force of the storm is sometimes very unequal at different places, situated in nearly the same part of its track, and such inequality, as we have before intimated, necessarily pertains to two places, one of which is near the centre, and the other towards the margin of the route.

Of the multitude of facts by which these views might be illustrated, we will only state, that in the late hurricane at Barbadoes (that of August 1831), the trees near the northern coast of that island lay from N.N.W. to S.S.E., having been prostrated by a northerly wind in the earlier part of the storm, while in the interior and some other parts of the island, they were found to lie from south to north, having fallen in the later period of the gale. That after the same hurricane, advices that were received from the islands of St Croix and Porto Rico (which lay near the northern margin of its track), stated that no hurricane had been experienced at these islands; but it afterwards appeared that some portions of these islands had suffered damage from this hurricane in the night of the 12th to 13th of August, two days after it passed over the island of Barbadoes.—That the sea-islands which border the coast of Georgia and the Carolinas, are known to suffer greatly from these tempests, while little or no injury is sustained in the interior at the distance of a few miles from the coast. One of the most striking characteristics of these storms, is the *heavy swell* which in open sea is often known to extend itself on both sides of the track, entirely beyond the range of the gale by which it was produced. The last hurricane to which we have alluded, threw its swell with tremendous force upon the northern shores of Jamaica, having passed to the northward of that island.

So strong is the influence of our established modes of thinking on this subject, that it seems to be difficult, even for those who admit the rotative character of these hurricanes, to understand correctly the true bearing and relations of the different phases of the wind, which are presented as two or more points or places visited by the same storm, unless the subject has been thoroughly and carefully studied. Speculative opinions, also, upon the course of a storm, are usually, if not always, founded upon the erroneous notion of a rectilinear course in the wind. In the accounts received of the hurricane at Barbadoes on the 3d of September 1835, which raged for a few hours from E.N. E., fears were expressed for the safety of the islands to the northward; but subsequent intelligence from Guadaloupe and Martinico, shewed that the gale had not extended to these islands. Had the direction and changes of the wind in this storm been viewed in their true relations, it would have been perceived that the heart of the gale must have passed to the southward of Barbadoes; and, as a general rule in the West India latitudes, where the onset of the storm is found to be in the general direction of the trade-wind, or more eastward, the observer may consider himself as under the northern verge of the gale; but if the onset of the gale be from the north-westward, veering afterwards by west to the southern quarter, the heart of the storm will be found to have passed to the northward of the point of observation, the latter being under the southern margin of the gale.

In order to illustrate the foregoing statements, I annex a chart of the Western Atlantic, on which is delineated the route of several hurricanes and storms, as derived from numerous accounts which are in my possession, by which their progress is specifically identified from day to day, during that part of their route which appears on the chart.

The route designed as No. I, is that of the hurricane which visited the islands of Trinidad, Tobago, and Grenada, on the 23d of June 1831. Pursuing its course through the Caribbean Sea, it was subsequently encountered by H. M. Schooner Minx, and other vessels, and its swell was thrown with great force upon the south-eastern shores of Jamaica on the 25th, while passing that island, where the wind, at this time, was light from the northward. After sweeping through the Caribbean Sea, this hurricane entered upon the coast of Yucatan, on the night of June 27th, having moved over the entire route from Trinidad to the western shore of the Bay of Honduras in little more than one hundred hours, a distance of about seventeen hundred nautical miles, which is equal to nearly seventeen miles an hour. I have no account of this storm after it crossed the peninsula of Yucatan, and it is probable that it did not again act with violence upon the ocean level. Its course or track to Honduras was N. 74° W.

Track No. II, that of the memorable hurricane which desolated Barbadoes on the night of August 10. 1831, and which passed Porto-Rico on the 12th, Aux-Cayes, and St Jago de Cuba on the 13th, Matanzas on the 14th, was encountered off the Tortugas on the 15th; in the Gulf of Mexico on the 16th, and was at Mobile, Pensacola, and New Orleans on the 17th; a distance of 2000 nautical miles in about 150 hours, equal to something more than 13½ miles an hour.* Its course, until it crossed the tropic of Cancer, was N. 64° W., or W. N. W. nearly. In pursuing its northern course, after leaving the ocean level, it must have encountered the mountain region of the Alleganies, and was perhaps disorganized by the resistance opposed by these elevations. It appears, however, to have caused heavy rains in a large extent of country lying north-eastward of the Gulf of Mexico.†

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* Mr Purdy states that this gale was felt at Natches, 300 miles up the Mississippi.

† In an article which was published in the April number of the *American Journal of Science*, I attempted to shew that *storms and hurricanes consist in the regular gyratory motion or action of a progressive body of atmosphere*, which action is the sole cause of the violence which they may exhibit; and that the storms of the Atlantic Ocean are *drifted* in a determinate direction, conforming to that of the general atmospheric current of the region in which they occur. The late hurricane in the West Indies, having from its peculiar violence attracted considerable attention, I am induced to offer you the following notices of its appearance and progress, which have been obtained from various sources.

The earliest accounts are from the Island of Barbadoes, where the hurricane raged with great violence on the night following the 10th of August. On the 11th it passed over the islands of St Vincent, and St Lucia, extending a portion of its influence to Martinico and the neighbouring islands on the north, and to Grenada on the south, but exhibiting its principal violence

Track No. III. is that of the destructive hurricane which swept over the Windward Islands on the 17th of August 1827;

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between 12° 30′, and 14° 30′ of north latitude. On the 12th it arrived on the southern coast of the Island of Porto Rico. From the 12th to the 13th it swept over the island of Hayti or St Domingo, and extended its influence as far southward as Jamaica. On the 13th it raged on the eastern portion of Cuba, sweeping in its course over large districts, if not the whole of that extensive island. On the 14th it was at Havanna, towards the west end of the same island. Of its progress on the 15th we have no distinct accounts,* but on the 16th and 17th it arrived on the northern shores of the Gulf of Mexico, in about the 30th degree of north latitude, raging simultaneously at Pensacola, Mobile, and New Orleans, where its effects were continued till the 18th; thus having occupied a period of six days in its passage from Barbadoes to New Orleans.

From the coast of the Gulf of Mexico the storm entered upon the territories of the adjoining States, where it appears to have spent itself in heavy rains. If its peculiar action was longer continued, it must have been only in the higher atmosphere, as we have no account of any violent effects at the surface nearer than the southern States.

When accounts of hurricanes were formerly received, as occurring at different Islands, on various dates, with marked differences also in the direction of the wind, it was taken for granted that those violent winds were *rectilinear* in their course, and that such accounts, in most cases, related to different storms. We now discover, however, that there is no difficulty in tracing these storms successively from one island or locality to another, and the direction of the wind at any one point or place is found to have no connexion with the general progress or direction of the storm.

At most of the islands, during the late hurricane, the winds in the earlier part of the storm were from a northern quarter, and in its later periods, from a southern quarter of the horizon; from which it results that the gyratory action was from *right to left*, as in the storms which pass to the northward of the great islands, and along our Atlantic coast. The distance passed over by the storm in its passage from Barbadoes to New Orleans, is equal to *twenty-three hundred statute miles*. The time of passage being six days, gives an average rate of about sixteen miles an hour, which accords with the rate of progress which I had previously ascribed to the storms of that region.

This hurricane appeared in a more southern latitude than those which are described in my article before mentioned, but pursued the same general direction as that which occurred at the same season in 1830, passing over or to southward of the great islands, and across the Gulf of Mexico, with a course curving northwardly as it approached the American coast. Hence it follows, that its atmosphere must have subsequently passed over a considerable portion, if not the whole, of the Atlantic States, according to the prevailing tendency of the general atmospheric current in this part of the globe. In its progress from Barbadoes to New Orleans, the storm was constantly enlarg-

* On the 15th the gale was encountered off the Tortugas.

visited St Martin's and St Thomas's on the 18th; passed the north-east coast of Hayti on the 19th; Turks' Island on the 20th; the Bahamas on the 21st and 22d; was encountered off the coast of Florida and South Carolina on the 23d and 24th; off Cape Hatterass on the 25th; off the Delaware on the 26th; off Nantucket on the 27th; and off Sable Island, and the Porpoise Bank, on the 28th. Its ascertained course and progress is nearly 3000 miles,* in about eleven days; or at the average rate of about eleven miles an hour. The direction of its route before crossing the tropic, may be set down at N. 61° W., and in lat. 40° while moving eastward, of N. 58° E.

Track No. IV. is that of the extensive hurricane of September 1804. It swept over the Windward Islands on the 3d of that month; the Virgin Islands and Porto Rico on the 4th; Turks' Island on the 5th; the Bahamas and Gulf of Florida on the 6th; the coast of Georgia and the Carolinas of the 7th; the great bays of Chesapeake and Delaware, and the contiguous portions of Virginia, Maryland, and New Jersey, on the 8th; and the states of Massachussetts, New Hampshire and Maine on the 9th; being on the highlands of New Hampshire, a violent snow-storm. The destructive action of this storm was widely extended on both sides of the track indicated upon the chart, and the same fact pertains, in a greater or less degree, to the other storms herein mentioned. It appears to have passed from Martinico, and the other Windward Islands, to Boston in Massachusetts, by the usual curvilinear route in about six days; a distance of more than 2200 miles, at an average progress of about 15½ miles per hour.

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ing in its dimensions and sphere of action, which is shewn by its increasing duration as it proceeded westward, as well as by other evidence.

It is perhaps worthy of notice, that the peculiar aspect of our atmosphere, together with the unusual colour and appearance of the sun, which excited so much attention a few weeks ago, was exhibited a few days after the occurrence of the hurricane at Barbadoes; and at Mobile and New Orleans, was the immediate precursor of the storm.

N. B.—To the list of localities in which the *second* hurricane of August 1830, exhibited its violence, as published in the Journal of Science, that of Martinico may be prefixed; at which island the southern margin of that storm shewed itself on the night of the 19th–20th of that month.

* All the distances are expressed in nautical miles.

Track No. V. represents the route of the hurricane which ravaged the islands of Antigua, Nevis, and St Kitt's, on the afternoon and night of August 12. 1835; St Thomas, St Croix, and Porto Rico on the 13th; Hayti and Turks' Island on the 14th; the vicinity of Matanzas and Havanna on the 15th; was encountered off the Tortugas in the Gulf of Mexico on the 16th; in lat. 27° 21′ long. 94°, and other points on the 17th and 18th; and also at Metamora on the coast of Mexico (lat. 26° 04′) on the 18th, where it was most violent during the succeeding night.* This storm is remarkable, as moving more directly, and farther to the west, than is usual for storms which pass near the West India Islands, it having reached the shores of Mexico before commencing its sweep to the northward. Its course, so far as known, is N. 73° W. Its progress more than 2200 miles in 6 days, which is nearly equal to 15½ miles per hour.

Track No. VI. is that of the memorable gale of August 1830, which, passing close by the Windward Islands, visited St Thomas's on the 12th; was near Turks' Island on the 13th; at the Bahamas on the 14th; on the gulf and coast of Florida on the 15th; along the coast of Georgia and the Carolinas on the 16th; off Virginia, Maryland, New Jersey, and New York on the 17th; off George's Bank and Cape Sable on the 18th; and over the Porpoise and Newfoundland Banks on the 19th of the same month, having occupied about seven days in its ascertained course from near the Windward Islands, a distance of more than 3000 miles—the rate of its progress being equal to 18 miles an hour.† If we suppose the actual velocity of the wind, in its rotatory movement, to be five times greater than this rate of progress, which is not beyond the known velocity of such winds, it will be found equal, in this period, to a rectilineal course of 15,000 miles. The same remark applies, in substance, to all the storms which are passing under our review. What stronger evidence of the rotative action can be required, than is afforded by this single consideration.

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* Since the above was written, it is ascertained that this storm also passed over Galveston Bay, on the coast of Texas, where the hurricane blew with violence from the north-east, while at the mouth of the Mississippi and along the northern shores of the gulf, the gale was not felt. Such facts appear quite sufficient to overthrow the hypothesis of Franklin relating to north-east storms, and are equally fatal to the more common theories. At Galveston this storm, in passing over, veered by east to the south-east; the *rationale* of which may be made evident by drawing a line through the northern side of the figure on the chart, parallel to the track of the storm. A little further attention to the figure will also illustrate the general character of the *northers* which are so common on the coast of Mexico, during a considerable portion of the year.

† For a more extended notice of this storm, see American Journal of Science, vol. xx. pp. 34–38.

Róute No. VII. is that of an extensive gale, or hurricane, which swept over the western Atlantic in 1830, and which was encountered to the northward of the West India Islands on the 29th of September. It passed on a more eastern route than any which we have occasion to describe, to the vicinity of the grand Bank of Newfoundland, where it was found on the 2d of October, having caused great damage and destruction on its widely extended track to the many vessels which fell on its way. Its course is quite analogous to that which we have considered as having been probably pursued by the hurricane of October 3. 1780. The ascertained route may be estimated at 1800 miles, and the average progress of the storm at 25 miles an hour.

Route No. VIII. is that of a much smaller, but extremely violent hurricane, which was encountered off Turks' Island on the 1st of September 1821; to the northward of the Bahamas and near the latitude of 30° on the 2d; and on the coast of the Carolinas early in the morning of the 3d; and from thence, in the course of that day, along the sea-coast to New York and Long Island, and which, on the night following, continued its course across the states of Connecticut, Massachusetts, New Hampshire, and Maine. We are not in possession of accounts by which its farther progress can be successfully traced.* The diameter of this storm appears not to have greatly exceeded 100 miles; its ascertained route and progress, is about 1800 miles in 60 hours; equal to 30 miles an hour.

The last mentioned route may also be considered to be nearly the same as that of a similar, but less violent storm, which swept along the same portion of the coast of the United States on the 28th of April 1835.

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* The phenomena and progress of this storm have been more fully noticed in Silliman's Journal, vol. xx. pp. 24–27.

No. IX. represents the route of a violent and extensive hurricane, which was encountered to the northward of Turks' Island on the 22d of August 1830; northward of the Bahamas on the 23d; and off the coast of the United States on the 24th, 25th, and 26th of the same month.

Much damage was done on the ocean by this storm: but it scarcely reached the American shores. Its duration off this coast, was about 40 hours, and its progress appears to have been more tardy than that of some other storms.

No. X. represents the track of a violent hurricane and snow-storm, which swept along the American coast from the latitude of 30° N. on the 5th and 6th of December 1830.

The last-mentioned track also corresponds to that of another storm of like character, which swept along the sea-coast on the 13th, 14th, and 15th of January 1831. These violent winter storms exhibit nearly the same phases of wind and general characteristics, as those which appear in the summer or autumn.

Track No. XI. represents a portion of the general route of the violent inland storm which swept over the lakes Erie and Ontario on the 11th of November 1835. This storm was very extensive, spreading from the sea-coast of Virginia into the Canadas, to a limit at present unknown. The anterior portion of this gale was but moderately felt, and its access was noted, chiefly, by the direction of the wind, and the great fall of the barometer; the violence of the storm being chiefly exhibited by the posterior and colder portion of the gale, as is common with extensive overland storms. The regular progression of this storm in an easterly direction is clearly established, by facts collected by the writer, from the borders of Lake Michigan to the Gulf of St Lawrence and the sea-coasts of New England and Nova Scotia.

We have thus given a summary description of the route of twelve storms, or hurricanes, which have visited the American coasts and seas, at various periods, and at different seasons of the year. The lines on the chart, which represent the routes, are but approximations to the centre of the track, or course, of the several storms; and the gales are to be considered as extending their rotative circuit from 50 to 300 miles, or more, on each side of the delineations; the superficial extent of the storm being estimated both by actual information and by its duration at any point near the central portion of its route, as compared with its average rate of progress. The figure which appears upon the chart, on Tracks No. I, V, and VII, will serve, in some degree, to illustrate the course of the wind in the various portions of the superficies covered by the storm, and also to explain the changes in the direction of the wind which occur successively at various points, during the regular progress of the gale. The dimensions of the several storms appear also to have gradually expanded during their course.

Storms of this character do not often act with great violence on any considerable extent of interior country to which they may arrive. Even upon the coasts on which they enter, such violence is not often experienced under the posterior limb of the gale which sweeps back from its circuit over the land, the usual woodlands and elevations being a sufficient protection. Often, indeed, the interior elevations afford such shelter as entirely to neutralize the effect of the wind at and near the surface, and the presence and passage of the hurricane is, in such cases, to be noted chiefly by the unusual depression, which the great whirling movement of the incumbent stratum of air produces in the mercury of the barometer, which thus indicates the presence or passage of the hurricane, in positions where the force of the wind is not felt at all, or only with a moderate degree of violence. The action of these storms appears, indeed, to be at first confined to the stratum or current of air moving next the earth's surface, and they seldom, while in this position, appear to exceed a mile or so in altitude; and the course of the next highest or overlaying stratum does not, in these cases, seem to be at all affected by the action of the storm below. During the progress, however, by the influence of high land and other causes, the storms often become transferred, in whole or in part, to the next higher stratum of current. Thus we sometimes see a stratum of clouds moving with the full velocity of a violent storm, while the stratum of surface wind is nearly at rest, or moves with its ordinary velocity; and thus, also, it happens that balloons, ascending under such circumstances, are carried forward with a velocity of from 60 to 100 miles an hour. The foregoing remarks are by no means hypothetical, but are the result of long continued observation and inquiry.

It will hardly escape notice that the track of most of the hurricanes, as presented on the chart, appears to form part of an elliptical or parabolic circuit, and this will be more obvious if we make correction, in each case, for the slight distortion of the apparent course in the higher latitudes, which is produced by the plane projection. We are also struck with the fact that the vertex of the curve is uniformly found on or near the 30th degree of latitude. In connection with this fact it may also be noted, that the latitude of 30° marks the external limit of the trade winds, on both sides of the equator ; and, perhaps, it may not prove irrelevant to notice, even further, that by the parallel of 30°, the surface area, as well as the atmosphere of each hemisphere, is equally divided ; the area between this latitude and the equator being about equal to that of the entire surface between the same latitude and the pole. It is not intended, however, to make these facts the basis of any theoretical inductions on the present occasion.

A variety of deductions may be drawn from the general facts which we have stated, some of which, though deeply interesting to the philosopher and votary of science, might be out of place in this short memoir. For ourselves, we disclaim any bondage to existing theories in meteorology ; and shall, on the present occasion, only proceed to notice a few of the more practical inferences which, to navigators and others, may, perhaps, be of no doubtful utility.

1. A vessel bound to the eastward between the latitudes of 32° and 45° in the western part of the Atlantic, on being overtaken by a gale which commences blowing from any point to the eastward of S.E. or E.S.E., may avoid some portion of its violence, by putting her head to the northward, and when the gale has veered sufficiently in the same direction, may safely resume her course. But by standing to the southward under like circumstances, she will probably fall into the heart of the storm.

2. In the same region, vessels, on taking a gale from S.E, or points near thereto, will probably soon find themselves in the heart of the storm, and after its first fury is spent, may ex-

pect its recurrence from the opposite quarter. The most promising mode of mitigating its violence, and at the same time shortening its duration, is to stand to the southward upon the wind, as long as may be necessary or possible; and if the movement succeeds, the wind will gradually head you off in the same direction. If it becomes necessary to heave-to, and the wind does not veer, be prepared for a blast from the north-west.

3. In the same latitudes a vessel scudding in a gale, with the wind at east or north-east, shortens its duration. On the contrary, a vessel scudding before a south-westerly or westerly gale, will thereby increase its duration.

4. A vessel which is pursuing her course to the westward or south-westward, in this part of the Atlantic, meets the storms in their course, and thereby shortens the periods of their occurrence, and will encounter more gales in an equal number of days than if stationary, or sailing in a different direction.

5. On the other hand, vessels, while sailing to the eastward or north-eastward, or in the course of the storms, will lengthen the periods between their occurrence, and consequently experience them less frequently than vessels sailing on a different course. The difference of exposure which results from these opposite courses on the American coast may in most cases be estimated as nearly two to one.

6. The hazard from casualties, and of consequence the value of insurance, is enhanced or diminished by the direction of the passage, as shewn under the two last heads.

7. As the ordinary routine of the winds and weather in these latitudes often corresponds to the phases which are exhibited by the storms as before described, a correct opinion, founded upon this resemblance, can often be formed of the approaching changes of wind and weather, which may be highly useful to the observing navigator.

8. It will be perceived from the foregoing facts, that the occurrence of a storm at a particular locality has no immediate connection with astronomical periods, such as the changes of the moon, or the time of the equinoxes.

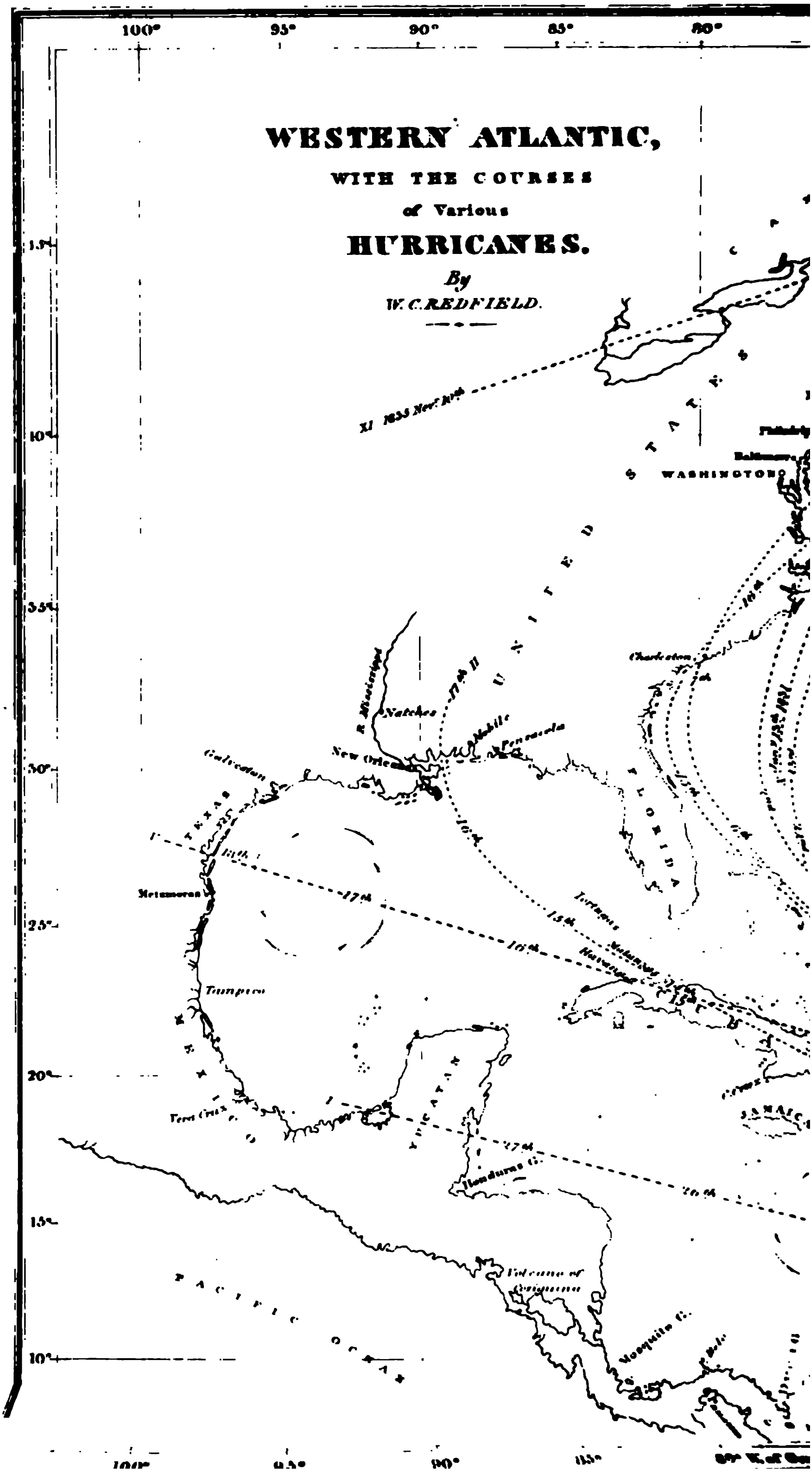
9. A due consideration of the facts which have been stated, particularly those under our twelfth head, will inspire additional confidence in the indications of the *barometer*, and these

ought not to be neglected, even should the fall of the mercury be unattended by any appearances of violence in the weather, as the other side of the gale will be pretty sure to take effect, and often in a manner so sudden and violent as to more than compensate for its previous forbearance. Not the least reliance, however, should be placed upon the prognostics which are usually attached to the scale of the barometer, such as Set-Fair, Fair, Change, Rain, &c., as in this region at least, they serve no other purpose than to bring this valuable instrument into discredit. It is the mere rising and falling of the mercury which chiefly deserves attention, and not its conformity to a particular point in the scale of elevation.

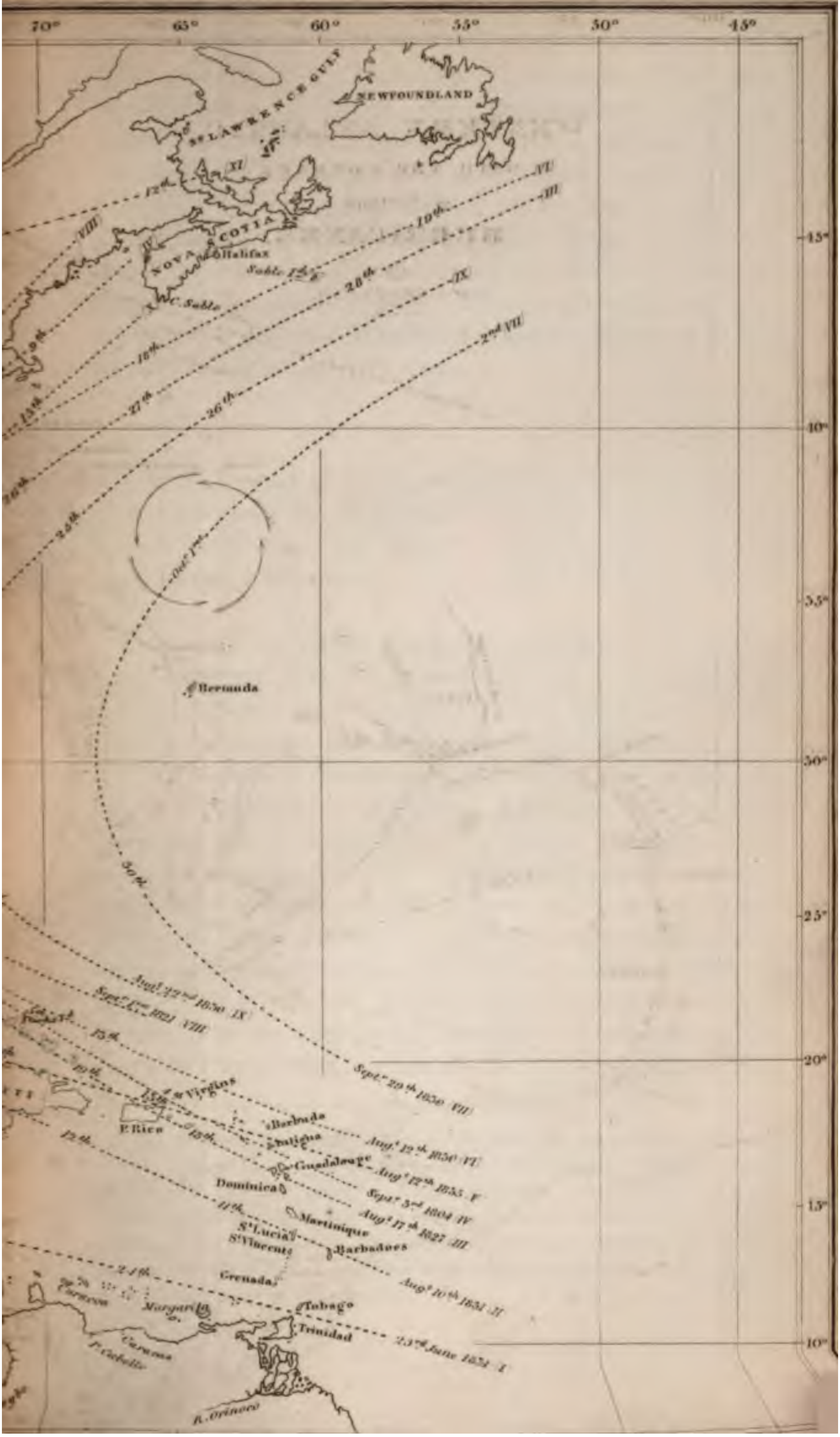
10. These practical inferences apply in terms chiefly to storms which have passed to the northward of the 30th degree of latitude on the American coast, but, with the necessary modification as to the point of the compass, which results from the westerly course pursued by the storm while in the lower latitudes, are, for the most part, equally applicable to the storms and hurricanes which occur in the West Indies, and south of the parallel of 30°. As the marked occurrence of tempestuous weather is here less frequent, it may be sufficient to notice, that the direction of the winds of the West Indian seas is from 8 to 11 points of the compass *more to the left* than on the coast of the United States in the latitude of New York.

Vicissitudes of winds and weather on this coast which do not conform to the foregoing specifications are more frequent in April, May, and June, than in other months. At this season it is not uncommon to find a regular current of easterly wind prevailing for many days, producing sometimes heavy rains, and always an elevated state of the barometer. Easterly or southerly winds, under which the barometer rises or maintains its elevation, are not of a gyratory or stormy character; but such winds frequently terminate in the falling of the barometer, and the usual phenomena of an easterly storm.

The typhoons and storms of the China sea and eastern coast of Asia appear to be similar in character to the hurricanes of the West Indies, and the storms of the North American coast, when prevailing in the same latitudes. There is reason to believe that the great circuits of wind, of which the trade winds


WESTERN ATLANTIC,
WITH THE COURSES
of Various
HURRICANES.
By
W. C. REDFIELD.
100°
95°
90°
85°
80°
45°
40°
35°
30°
25°
20°
15°
10°
XI 1835 Novr 18th
UNITED STATES
WASHINGTON
Baltimore
Charleston
R. Mississippi
Natchez
New Orleans
Mobile
Pensacola
Galveston
TEXAS
FLORIDA
Matamoras
Tampico
MEXICO
Vera Cruz
YUCATAN
Honduras C.
Havana
Matanzas
Tortugas
JAMAICA
Volcano of Guatimala
Mosquito C.
PACIFIC OCEAN
17th
16th
15th
10th
20th
80° W. of Gr.

Edinr New Phil. Jour. Vol. XXIV. p. 327.



form an integral part, are nearly uniform in all the great oceanic basins; and that the *course* of these circuits, and of the stormy gyrations which they may contain, is, in the southern hemisphere, in *counter direction* to those north of the equator, producing a corresponding difference in the general phases of storms and winds in the two hemispheres. From the foregoing results we infer the value and importance of correct marine reports relating to violent gales. These reports should always comprise the *date*, the *latitude* and *longitude*, and the *principal direction and changes of the wind.*

On the Differences of the Laws regulating Vital and Physical Phenomena. By WILLIAM B. CARPENTER, M. R. C. S., late President of the Royal Medical and Royal Physical Societies, Edinburgh.* Communicated by the Author.

1. ALL the knowledge which man possesses of the external world is derived from the impressions made by matter, under some of its forms, upon his organs of sense, which excite sensations, perceptions, and reasoning processes in his mind.

2. Of *matter*, in the abstract, man has no definite idea; he judges of it only by its properties; and as his notion of these properties results simply from the changes produced in his organs of sensation, the term "properties of matter" expresses nothing else than the relations between matter and the percipient mind. And just as the occasional varieties in the conformation of the organs of the senses lead to a different appreciation of certain of these properties (*e. g.* as regards colour or sound), in certain minds, so is it possible to conceive that beings might be created whose organs should take cognisance of a set

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* The following paper constitutes a portion of the essay to which was recently adjudged the prize annually raised by the contributions of the students, and awarded by the Professors, of the University of Edinburgh. Circumstances, which it is unnecessary to specify, have rendered it desirable not to publish, in its present form, the division of the essay comprehended in § 9–20; an abstract of which has been introduced, however, for the purpose of shewing the course of the argument.

of properties appertaining to matter, of which man, as at present formed, has no conception.

3. The *properties of matter* may be classed in various ways;[*] some are *universally* possessed by all matter which comes under our cognisance; and our most definite notion of matter is therefore compounded of the ideas which they excite: even these, however, do not lead us to the knowledge of the entity or substance to which they belong; and it is generally allowed that "we are entirely ignorant of the real constitution of matter, the cause from which the most universal and essential of its properties arise." Besides these universal properties, however, there are others *partially* distributed among material bodies, of whose cause we are equally ignorant, but whose varieties (mostly recognisable by the organs of special sense) lead to all our knowledge of the particular forms and qualities of matter.[†]

4. Our ideas of the external or evident characters of material bodies, are simply derived from the changes produced by them (whilst themselves inactive) upon the organs of sense. It is probable that these primary impressions are of a material nature; and that the human mind, in the present stage of its existence, cannot take cognisance of any of the properties of matter, except by such material changes propagated from the extremities of the nervous system to the central sensorium.[‡] If this be

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[*] Of the ancient metaphysical distribution of the properties of matter into the *primary* or *essential*, and the *secondary* or *accidental*, it is scarcely necessary here to treat, since this distribution was not founded on the respective *universality* or *partiality* of these properties, but on their supposed connection with what was regarded as the fundamental or primary character of matter, viz., its occupation of space. (See Adam Smith on the External Senses, and Prichard on the Vital Principle, p. 43.)

[†] The sense of Touch, considered in its simple and passive state, leads only to the idea of *resistance;* and hence to the knowledge of the universal properties of matter. When actively employed in combination with other more special sensations, it leads to the appreciation of those external forms and characters of matter which are liable to constant variation.

[‡] "The impressions excited in us by external objects are the results of certain actions and processes, in which sensible objects and the material parts of ourselves are directly concerned." Herschel's Preliminary Discourse, p. 118.

the case, what is true of a great number of instances may be stated as a general proposition,—that none of the properties of a material body can be recognised by the mind, except when that body is placed in relation with some other, and an action of some kind occurs between them. That this is true of many properties, as that of gravitation, is self-evident. If but one mass of matter existed in the universe, it might be endowed with all the properties which we are accustomed to regard as essential to matter; and yet, from the property of gravitation never being brought into action, the mind would remain ignorant of it.

The same may be said of electricity, which is probably to be regarded not as a distinct entity, but as a universal property of matter.

In these latter cases, the body itself must undergo a change in state or situation, in order that its occult properties may be manifested, and until it has been placed in circumstances by which the existence of such properties can be tested, we have no means of judging of their presence or absence.

Thus, supposing a new metal to be discovered, we should not be able to say *a priori* whether or not it had magnetic properties; we could only determine this by experiment. Further, until our experiments have been varied in every conceivable manner, we have no right to judge of the absence of such a property; thus Faraday has shewn that it is by no means impossible that all metals are possessed of magnetic properties, within a limited range of temperature.

5. Still, therefore, keeping in view the statement with which we set out, that the term "property of matter" expresses no more than a certain relation between matter and mind, we may regard it as implying a capability of producing or of undergoing a change when the body is placed in conditions adapted to manifest it.* If this change be immediately produced on any of the organs of sense, when the body is brought into relation with it, the property may be regarded as of an *evident* nature; if an action with some other body be required to manifest the property of the senses, the property may be designated as *occult.* This distinction though not perhaps metaphysically correct,

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* "If all the objects in nature remained constantly at rest, it is very evident that we could have no notion of any property of matter whatever." Brown's Lectures, p. 26.

will be found useful for practical purposes, and will be frequently referred to in subsequent observations.

6. The manifestation of the properties of matter, in the actions which bodies, placed in certain relations to one another, present to our observation, gives rise to our idea of *power*. Thus, when two bodies possessed of the property of gravitation are placed within the sphere of each other's attraction, the *power of gravity* is developed, and manifested by their approach to one another. Or when a piece of iron is brought near a magnet, the electric equilibrium of the former is disturbed, and the attraction caused by the opposite electric state of the neighbouring parts of the two bodies gives rise to their tendency to approximate; or in common language, the *magnetic power* causes their motion towards each other. Of the abstract nature of any *power*, as of matter itself, we know nothing; it is only recognisable by its effects; but it is, as we have seen, to be ultimately referred to the *properties* of matter, which may be regarded as axioms or postulates in any course of reasoning founded upon them. Of power as a *cause* of *effects*, it is generally acknowledged that we judge of it only by its invariable connection with subsequent phenomena.

7. To the term *principle* no very definite meaning can be attached. It has been remarked that " this word, characteristic of a less advanced state of science, has been generally employed (as the final letters of the alphabet are used by algebraists) to denote an unknown element, which, when thus expressed, is more conveniently analysed."[*] Thus we speak of the principle of electricity, or of the principle of magnetism, as the unknown causes of certain phenomena that are as yet imperfectly understood. When the laws of these phenomena, however, are perfectly ascertained, they terminate in referring all of them to simple properties of matter, from which they may be deduced by demonstrative reasoning, just as all geometrical theorems are founded on the axioms first assumed. It has been in the science of life that the term Principle has been most used, and most abused; the conditions of vital phenomena are not yet determined with sufficient precision to enable us to refer all ob-

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[*] Mayo's Physiology, p. 2.

served facts through the medium of general laws to simple vital properties; and the term *vital principle* has been used as a convenient expression for the sum of the unknown *powers*, which are developed by these properties in action. This at least is its true signification among those who have not regarded it as a distinct *intelligent agent.* But when these vital properties shall have been so far unveiled, and their laws so fully ascertained that we can reason deductively from them, then the term Vital Principle will become wholly useless, except as a comprehensive expression of the *sum* of the properties of living organized matter not possessed by bodies in general. But perhaps it may be speculated without improbability that the *vital* and *physical* properties of matter may ultimately be shewn to result from some higher, more general quality; an advance in the path of philosophy, should it ever be proved, far beyond any which has been already attained, or which we have in immediate prospect.

8. The term *law*, which has been already more than once employed, expresses the *conditions of action* of the properties of matter. The Divine Creator of the universe "has, by creating his materials, endued with certain fixed qualities and powers, impressed them in their origin with the *spirit*, not the *letter*, of his law, and made all their subsequent combinations and relations inevitable consequences of this first impression."* In our study of the phenomena of nature, it is our object to ascertain their laws by the inquiry into the conditions under which the occurrences present themselves; and a *law* deduced from this source is nothing more than a general expression of the conditions common to a certain class of phenomena, leading us to the belief, that, under the same conditions, the same phenomena will constantly occur. When this is found to be the case by the experimental application of the law to unknown cases, the law is said to be verified, and it may then fairly rank as a general fact to be included with others of like standing in a still higher expression of the conditions common to all these, and therefore to all the particular instances included in them. By successive generalizations of this nature, we aim to ascend

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* Herschel's Preliminary Discourse, p. 37.

from the most complicated and restricted to the most simple and universal statement of the phenomena of the universe; and in so far as this is attained in every science, giving us the means not only of explaining new phenomena as they arise, but of predicting otherwise unexpected occurrences, *that* science may be regarded as perfect.

Summary of Sections 9–20.

The object of these was to shew that there is nothing essentially different in the *character* of the laws regulating vital and physical phenomena, either as to their comprehensiveness, their uniformity of action, or the mode in which they are to be established by the generalization of particular facts. Matter, in that form usually denominated *inorganic*, is possessed of *properties* which render it capable of performing a great variety of *actions*, and of developing many *powers;* and the nature of the properties determines the conditions of action, or, in other words, the *laws of physical phenomena.* According to our means of observing the actions dependent upon the different properties in an insulated form (whether that insulation be natural or artificially contrived) are we able to arrive at the fullest knowledge of these properties; and where the combinations, under which natural phenomena so frequently occur, are placed (as in meteorological science) beyond our power of analysing, although we may, through the assistance afforded by collateral sciences, determine most or all of the laws in operation, we are unable to apply these laws to the complete explanation of individual phenomena, or to the prediction of unexpected occurrences, owing to our ignorance of the mode in which they are combined, and the relative parts they play in the production of the effects in question.

On the other hand, *living organized matter* is possessed of properties (apparently superadded to those of inorganic matter) which render it capable of performing a great variety of *actions*, and of giving rise to many *powers;* and the nature of the properties determines the conditions of action, or, in other words, the *laws of vital phenomena.* The peculiar difficulties which beset the investigation of these laws have greatly retarded our acquaintance with them, and, until a comparatively recent period, led to the belief that the inductive process was not applicable to their elucidation. It is obvious that these difficulties are in many respects similar to those which present themselves in the science of meteorology; but we have not in the science of life the same assistance which meteorology has derived from the collateral branches of philosophy, since to whatever degree the physical properties of matter are concerned in the actions of life, it would be absurd in the present state of our knowledge to deny the existence of a class of *vital* properties essentially distinct from these, and never manifested but by matter in that form which is denominated *organized*, and which the skill of man can never hope to produce or imitate.

21. When we compare the constant changes which we encounter in living organized beings, with the inert state of inorganic matter, we are compelled to conclude, that to whatever

extent the forces which control the latter contribute to the action's going on in the former, there must be additional forces resulting from the operation of properties, to which we know nothing analogous elsewhere. The degree, however, in which these superadded properties harmonize or interfere with those common to other forms of matter, constitutes a fair and highly interesting branch of inquiry; and it is a question of a still higher nature, whether the existence of *vital* properties is to be regarded as the *effect* or the *cause* of the peculiar material constitution of the tissues which exhibit them? Each of these questions we shall now examine in some detail.

It would be useless to recapitulate the various opinions which have been at different times advanced regarding either of these subjects; different writers have espoused the opposite extremes, some maintaining that the actions performed by living beings are purely of a physical nature; others, that physical powers are not concerned in them; and others have even presumed upon the existence of a distinct intelligence, "Divinæ particula auræ," presiding over the affairs of each organism. The truth appears, with regard to this, in common with so many disputed questions, to be in the mean between the opposing extremes.

22. The actions performed by living beings, the sum of which constitutes their life, may be divided into three classes. In the *first* division may be included all those which are of a purely *physical* character, depending solely upon the common properties of matter, and performed by dead as well as by living organized substances, as long as no obvious change has taken place in their composition. The *third* division will comprehend all those changes which result strictly from the *vital* properties of the living tissues, and which, therefore, entirely cease at the moment of death. The *second* or intermediate division includes all the changes in which *physical* laws may be regarded as the immediate agents, but in which the conditions of their operation are determined by the action of vital properties. It will, we think, be found that all the changes by which the organism is immediately connected with the external world (whether those changes are a part of the *organic* or *animal* functions) belong to the *first* class; that the actions concerned in the conversion of the crude aliment, introduced from without into *organizable* materials, which may serve as the pabulum of the various tissues, are to be referred to the *second* class; whilst the act of *organization*, and the endowment of the newly

[illegible] with the study of vegetable life.

23. The properties of the elementary tissues of plants may be [illegible] with some degree of [illegible] these tissues are frequently but little prone to spontaneous decomposition after the loss of their vitality. [illegible] that the properties of elasticity, hygrometricity, and that peculiar modification of the latter which produces endosmose, are evidently the result of the mechanical arrangement of the particles of the tissues which exhibit them; since they are not dependent upon vitality for their existence, and may be imitated by artificial combinations of inorganic matter.* On the other hand, these tissues are also possessed in their living state of properties which, [illegible] main-tain those changes which essentially constitute the life of the plant, and without which the mere physical properties would be inert.

24. The first and most general of the actions of a vegetable organism consists in the absorption of nutrient materials from without. This absorption takes place during every period of the life of the plant from the first appearance of the embryo within the ovule to the final decay of the structure. Now, there would seem strong reason to believe that the mere act of absorption is due to the physical property already referred to, as the capability of producing endosmose. The succulent extremities of the spongioles in the perfect plant, or the tegumentary membrane of the rootless cryptogamia, serve as the medium required for the process; but it is important to keep in view,

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* Thin laminæ of baked pipe-clay are found to be excellent media for the production of endosmose; and porous limestones possesss a similar property in an inferior degree.—Dutrochet's Memoires, Anatomiques et Physiologiques, vol. i. p. 23, &c.

that, for its commencement and continuance, a constant difference is necessary in the density of the fluids on the two sides of the septum. This difference appears to be produced in the living plant by the admixture of *elaborated* or descending, with the *crude* or ascending sap; and in consequence, a portion of the former is excreted from the roots by the action of exosmose. The cessation of this action of admixture (a change obviously dependent upon vital actions) at the death of the plant, fully accounts for the non-continuance of endosmose, which might also be occasioned by the facility with which the delicate tissue of the spongioles undergoes disorganizing changes. It is also evident that the continuance of the vital processes going on in the leaves is necessary for the removal of the imbibed fluid as fast as it is absorbed by the spongioles, just as the combustion of the oil at the apex of the wick is necessary for its continued rise by capillary action beneath;* and it is now acknowledged that the motion of the sap in spring commences at the extremities of the branches, owing to the vital actions which result from the stimulus of heat and light, the endosmose not occurring at the roots until the superincumbent column of fluid has been drawn off. If these views be correct (and they are not negatived by the few instances of *selective* absorption which occur in plants, as these are readily explicable on this theory (*Edin. Med. and Surg. Journ.* xlviii. p. 24), it follows that we are to regard *absorption* as a change resulting primarily from the *physical* properties of the tissue, but dependent for its continuance upon actions of a *vital* nature in other parts of the organism.

25. In the early stage of vegetable existence, before the evolution of the organs, whose object is to convert the crude aliment into organizable materials, the embryo is supplied with nourishment already prepared by the parent. When, however, the plant has arrived at its full growth, its aliment is derived from the simplest materials, viz, water, carbon, and, in a few instances, azote; and it is in the conversion of these into the organic compounds which are found in the nutritious juices of plants, that we may recognise the action of physical laws,

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* Henslow's Botany, p. 179.

under those peculiar conditions which a living organism alone can supply. In so far as the skill of the chemist can imitate those conditions he may hope to produce similar combinations, and this has to a certain extent been already effected; but even those who believe that the existence of vital properties is the result of the organization of the tissue which manifests them, must despair of themselves effecting that process, which seems to be entirely dependent on the previous existence of some other organizing body. The three great steps in the function of assimilation (first, the absorption of aliment; *second*, its conversion into organizable materials or proximate principles; third, its organization and vitalization) present us with illustrations of the three classes of actions which we assumed as expressive of the mode in which the changes constituting life may be most simply and philosophically distributed. But the opinion here given with regard to the formation of organic compounds, which we have referred to the action of chemical affinity (believing the peculiarity of the results to be due to the mode in which the elements are presented to each other, rather than to any thing essentially different in the bond of union), is opposed to that of many eminent physiologists of the present day: and as it may be regarded as one of the most important questions in physiology, and one which the weight of the facts and arguments on either side renders peculiarly open to discussion, we shall devote some space to the consideration of it.

26. The question may be advantageously stated in its broadest form.—"Are the affinities which hold together the elements of living bodies, and govern the elaboration of *organic products*, similar to, or distinct from, those which preside over inorganic compounds?" On this question we find the greatest number of modern physiologists opposed in belief to the chemists of the present day; the former maintaining the existence of a distinct set of *vital affinities*, sometimes, and indeed generally, opposing the usual action of chemical laws; the latter believing, that, however at first sight the actions of vital chemistry may appear inexplicable by the known laws of affinity, they are to be ultimately explained on the same general principles. The physiologist points to the evident truth, that the tissues and fluids which maintain a certain composition whilst

possessed of vitality, speedily resolve themselves into new combinations, a fact so obvious that it has given rise to the well known definition of *life*, that is, the power by which decomposition is resisted. Hence it is inferred, that the affinities which hold together the elements during life are of a different nature from those which operate in producing their subsequent separation. Now, it may be replied to this argument, that no solid or fluid compounds which have a disposition to spontaneous decay after death, can continue to exist without change during life; that the activity of the processes of interstitial absorption and reposition seems to bear a pretty constant ratio in every case with the natural tendency to decomposition; and that the maintenance of the original combination is not so much owing to any thing peculiar in its *vital affinities*, as to the constant provision for the removal of particles in a state of incipient decay, and their replacement by others freshly united,—processes which are obviously dependent on vital actions for their continuance. Thus we find that all the most permanent parts of the animal frame, such as the massive skeletons of the polypifera, the calcareous tegument of the mollusca, the bony scales of fishes, all of which are believed by geologists to have remained almost unchanged for thousands of centuries, are completely extravascular in the living animal, undergoing no interstitial change when once formed; next to these in order of durability are the osseous structures of animals and the woody fibre of vegetables, whose connection with the circulating system seems rather adapted to meet the exigencies of growth, injury, or disease, than to maintain a constant change required by the tendency to decomposition. When we examine the softer tissues, on the other hand, we find that the rapidity of interstitial change fully compensates for the increased tendency to decay; and that if, from any cause, this change be prevented, decomposition and *consequent* loss of vital properties ensue, as in the case of spontaneous gangrene from obstructed circulation. It is interesting to remark also, that the liberation of carbonic acid, which, when it begins to take place after death, is one of the first signs of putrefaction, is the most constant and necessary excretion of the body during life. It might also be argued, that, independently of the changes already adverted to, by which the normal composition of the organic constituents is maintained, the circumstances under which these compounds exist in the living body differ in so many particulars from those of dead matter, that no conclusion could fairly be raised upon the fact of their spontaneous decomposition after death; since inorganic chemistry affords us numberless examples of such changes occurring under the influence of very slight variations in temperature, electricity, light, &c. Moreover, many proximate principles, when reduced by chemical means to their simplest form, are as permanent as the greater part of the inorganic compounds; and it is difficult to suppose that they retain their vital affinities after being subjected to chemical action.

Thus starch and gum may be converted into sugar by the operation of various acids; and sugar at moderate temperatures is as little liable to change as any saline compound.

27. It has been said by the physiologist, that the processes of vital chemistry are "not only inexplicable by, but manifestly opposed to, the known action of chemical and physical laws;" and it is therefore presumed that a distinct set of forces must be in operation, by which these changes are effected; which forces have received the name of vital affinities. This presumption carries with it a specious probability, and is not easy to be refuted by positive statements; we shall examine, however, whether the real probability does not support a contrary view. It may be well to repeat that the question we are now arguing relates only to the formation of *organic products*, such as gum, sugar, albumen, gelatine, &c., which are destined to be further organized—or such as urea, cholesterine, &c. which are to be thrown off from the system, and not to those which, like starch or fat, already present traces of structure, and have therefore partaken of those influences which we consider essentially vital.

In the first place, then, we may remark that it is rather premature to assert that any such changes in constitution are contrary to the known laws of chemical affinity; for no one who has watched the progress of science during the last few years, can hesitate in the belief, that we as yet know but little of those laws, compared with what future discoveries will reveal to us. The same assertion might have been made within a very recent period, with regard to many of those phenomena which we can now readily explain. Would it have been thought possible, for instance, by a chemist thirty years ago, that the same substance should act the part of an acid in one case, and a base in another?—that *water* should be possessed of such properties?—or still more that hydrochloric *acid* in combination with chloride of platinum should act as the *base* or electro-positive ingredient? Yet such are the facts. These would have appeared to a chemist at the commencement of the present century, totally inconsistent with what he knew of chemical action; but they are now readily comprehensible under laws which include all the facts hitherto ascertained. Or, to take a different illustration, would any electrician, twenty years ago, have supposed it consistent with physical laws, that a mechanical force, 50,000 times greater than that of gravity, may be instantaneously generated by the action of galvanism on a metallic alloy (as shewn by Sir J. Herschel), or that a feeble current of electricity, issuing from a single pair of plates, may generate (if properly applied) a magnetic force capable of sustaining many hundred pounds. The higher and more general are the laws we attain, the more do we find that they include facts, which at first sight appeared inconsistent with them; and unless a *distinct* set of *laws* could be established, regulating vital affinities, which has not yet been accomplished or even attempted, we are scarcely justified in assuming that these laws may not be accordant with those which we recognise elsewhere.

And this leads us to remark, in the second place, that whilst recent discoveries in inorganic chemistry have tended to widen very much our notion of the play of *chemical* affinities, the application of accurate and minute analysis to what have hitherto been considered the proximate principles derived from the organic world, has shewn that many, if not all of them, may be considered as binary combinations of other substances much simpler in their composition, and every fresh discovery is therefore tending to break down the barrier between the two classes of organic and inorganic bodies, as far as regards their method of chemical combination.*

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* Turner's Chemistry, p. 782, Prout's Bridgewater Treatise, p. 418, &c.

The chief ground for the assumption of a distinct set of vital affinities, appears to be, that whilst man has the power of effecting or controlling those changes which are produced by physical laws (so far as the materials concerned in them are within his reach), and can therefore imitate, to a great extent, the immense variety of combinations which the mineral kingdom affords,* he is at present unable to effect or control the action of similar materials, so as to produce any of the class of substances usually termed organic. Every fresh discovery, however, tends to prove that the powers *immediately* concerned, are, like the elements on which they act, the same in all cases; the difference in the effects produced being owing, not to any alteration in the physical properties of the constituents, but solely to the manner in which they are brought to bear upon one another. We cannot yet succeed in producing artificially any organic compound, even of the simplest kind, by directly combining its elements, because we cannot bring them together precisely in the requisite states and proportions; we do not see why it should be doubted; that if the elements could be so brought together by the hand of man, the result would be the same as the natural compound. For, as Dr Prout justly remarks, "the organic agent does not change the *properties* of the elements, but simply combines them in *modes* which we cannot imitate." If we once admit the doctrine of the substitution of vital for chemical affinity as the *immediate* agent in the production of organic compounds; we do not see how the conclusion can be avoided, that *all* the changes which take place in the living system, whether animal or vegetable, are to be referred to the same source, and that, in fact, chemical affinity has nothing to do with them—a conclusion to which we imagine that few would assent. Those who are acquainted with the influence of temperature, electricity, light, the *form* of the body operated on, and the *state* in which it is presented for combination—on chemical actions, must be well aware how greatly the effects are modified by slight differences in the conditions; and it scarcely seems too much to assume that, ignorant as we are of the nature of these conditions in the living organism, the acknowledged differences in the results may be dependent upon their variations; perhaps, also, other conditions whose nature is yet unsuspected may also have a share in their production.

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* In this point of view the recent experiments of Mr Crosse and others on artificial minerals, are highly interesting; no chemical laws previously known were adequate to account for the formation of many common mineral combinations.

Having now endeavoured to shew, by negative arguments, that we are not justified in assuming that the union of elements in organic compounds is effected by a novel kind of affinity, unknown in the inorganic world, we shall next briefly inquire what *positive* reason we have for believing that chemical affinity is the agent concerned in the formation of organic products. It would be endless to enumerate *all* the actions which we might, without passing beyond the limits of our absolute knowledge, adduce in support of our argument; but we may notice a few of the most remarkable. Many of the proximate principles of plants are mutually convertible; thus gum may be changed to sugar; and starch, which is a semi-organized form of gum, may be made to undergo a similar transformation, by the action of heat and sulphuric acid.

This is one of the remarkable instances of the catalytic action described by Berzelius,* which is common to organic and inorganic chemistry, and which is not yet comprehensible in the known laws of chemical affinity; the peculiarity of the action consists in the change produced by one compound upon another, without itself undergoing any alteration. Thus, in the present instance, the quantity of sulphuric acid undergoes no diminution, nor does it form any new combination.

Now, there are two distinct actions in this change, the rupture of the vesicles causing the liberation of the contained gum, and the conversion of the gum into sugar; the former is effected by the heat, the latter by the heat and acid combined. The process is one of the most common in the vegetable economy, being performed whenever an embryo begins to be developed, and makes use of the store of nourishment previously prepared by the parent, as in germination; but in the plant it is accomplished in a manner somewhat different. A secretion which has been termed *diastase* is brought to act on the fecula, and effects its conversion into sugar, just as the chemist may do by heat and sulphuric acid: this diastase is previously stored up in contact with the embryo, and when the vitality of the latter is excited by its appropriate stimuli, it is carried into the substance of the albumen. Diastase may be easily obtained from the tuber of the potato, where it surrounds the buds; and when the chemist employs it as a reagent, its effect on starch corresponds with that which it produces in the living plant, and with that of the purely chemical operation of sulphuric acid. Another example is afforded by the function of respiration,* which, although not yet perfectly elucidated, seems almost entirely under the control of physical and chemical laws. The object of this function is the excretion of superfluous carbon from the system, in a manner calculated to give rise to the development of heat. We shall not enter into the question at present most keenly debated, with regard to the form in which carbon exists in venous blood. This appears to have been recently decided by the experiments of Magnus; but there can be no question that, when that fluid is exposed to the air through the medium of a thin membrane, an interchange of ingredients takes place, carbonic acid being removed and oxygen absorbed.

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* Edinburgh Philosophical Journal, vol. xxi.

The removal of carbonic acid is proved by the experiments of Edwards on respiration in hydrogen; the absorption of oxygen (which cannot be detected *free* in arterial blood) by the predominance which often occurs of the quantity of oxygen which has disappeared over that of the carbonic acid which replaces it.

A constant absorption and exhalation of nitrogen also appears to take place. Now, all these facts are explicable upon the known laws of the mutual diffusion of gases. The most evident of these changes take place after the removal of blood from the animal, if it be exposed either directly or through a membranous septum to the contact of oxygen; and we can scarcely, therefore, view them in any other light than as chemical. But these changes, like others which result immediately from the relations between organized beings and the external world, although themselves resulting from physical laws, require for their continuance the assistance of actions dependent upon vital properties, and hence cease soon after general or somatic death, although they may continue so long as the necessary conditions can be artificially maintained. It has been considered by many writers that the excretion of carbon in respiration is due to a secreting action in the lungs; this we do not mean to deny, particularly as the elementary structure of the lungs corresponds precisely with that which is now known to be common to all secreting organs; but we think that from the function of respiration, which is essentially a chemical process, an argument may be drawn respecting the operation of chemical principles in the formation of other secretions, although their nature is at present so obscure. There can be no doubt, however, that they are modified by the electric state of the organs by which they are respectively formed; and this fact leads us to another very interesting series of details, from which it would appear that electricity is a most important, though little recognised agent, in the operations of organic chemistry.

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* We use the term in its most general sense, denoting the interchange of ingredients between the air and circulating fluid.

The late researches of Dr Faraday have fully proved the identity of electrical with chemical affinity, and that all chemical changes are attended with a disturbance of electric equilibrium. If therefore the changes occasioned by the growth of organized systems are immediately governed by laws similar to those of inorganic matter, we should expect to find that electricity is constantly being developed by them, in the same manner as we artificially obtain it by chemical decomposition or recomposition.*

There is no deficiency of evidence that this is the case. During the germination of seeds, the two principal changes are the rejection of carbonic and acetic acids; and it has been recently ascertained that there is at the same time a manifestation of electric action. The seed may indeed be considered an electro-negative system, retaining the bases and rejecting the acids; and it has been accordingly found that grains applied to the negative extremity of a voltaic pile, germinate much more rapidly than those uninfluenced; and that positive electrical influence retards the process. In like manner slightly alkaline solutions accelerate, and acids delay or altogether check it. In the later periods of vegetable growth, the contemporaneous chemical changes are by no means uniform in character; and it is probably from this cause that artificial currents of electricity do not seem to assist the growth of plants, although atmospheric electricity, which is undoubtedly much connected with the processes of vegetation, appears to accelerate it. That there is constant electric disturbances during the growth of plants, has been fully proved by the experiments of Pouillet; and by many writers, the changes produced by the exhalation of fluid, and the gaseous alterations effected by the leaves, are believed to be the main source of the constant variations in the electric state of the atmosphere.

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* M. Becquerel has described a most simple apparatus for the development of electricity, consisting merely of a syphon filled with fine sand, and having one leg filled with an acid, the other with an alkaline solution. These fluids meet at the most depending part of the tube, where there is an orifice plugged by a bit of asbestos, which conveys away the compound solution as fast as formed. Wires placed in the two legs indicate strongly opposed electrical states, and the voltaic current thus produced, continues until all the fluid elements have been united. It is impossible to consider this result without acknowledging the remarkable influence which *capillarity* must have over chemical action, a condition so evident and constant in organized beings.

The connection of *capillarity* with electric action has been already noticed; but some other facts may be briefly stated. Various substances having minute porous structure, possess the power of occasioning the union of oxygen and hydrogen at comparatively low degrees of heat; thus spongy platinum will produce this effect at common temperatures, and charcoal or porcelain biscuit at about 300°. It does not seem very clear to what this power is to be attributed; and we are almost equally in the dark regarding the phenomena of endosmose, in which electricity would appear to have some share, the known laws of capillary action not being adequate to explain some of the recently observed facts.*

Many facts corresponding with those to which we just now alluded as having been obtained with regard to the electrical state of different organs of animals, have been remarked in vegetables also. Thus it has been ascertained that wires passed into the pith and applied to the bark indicated opposite electrical states; and the same is true of the two extremities of most fruits. Some of the most interesting proofs of the occurrence of electric actions in plants are derived from the experiments of M. Becquerel and Mr Crosse, on the effect of currents of voltaic electricity of very feeble intensity, in producing the crystallization of many substances, which, from their insolubility, the chemist has been hitherto unable to procure in that form, but which occur abundantly in vegetables, such as silex, and the carbonate and oxalate of lime. Now, unless we suppose that vital affinity or action possesses this remarkable property in common with electricity, a supposition which appears entirely gratuitous, we cannot hesitate to set down the deposit of these salts in a crystalline state in the vegetable tissues, to the electricity developed by other chemical actions going on in the plant.

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* Cyclopedia of Anatomy, p. 110.

It is hoped that the foregoing statements will have established the probability (which is all that the present state of our knowledge on these subjects will allow us to assert) that the forces *immediately* concerned in the production of the changes of decomposition and recomposition in the living body, are the same as are in constant operation in the world around ; and that it is at any rate premature to assert that the operations of vital chemistry are directed by distinct laws, and due to new forces. Indeed, one at least of the writers who has employed the term vital affinity, would seem not to differ essentially from the opinions here given, since he considers,* that this property *modifies* (not supersedes) the chemical relations of the substances subjected to it. This modification we believe with Dr Prout to be due to the *mode* in which other vital properties (the *ensemble* of which seems to constitute the *organic agent* of Dr Prout) bring together the elements ; and if this be true, there can be no definite conception attached to the term *vital affinity* as distinct from chemical affinity.

We may also express our concurrence with the opinion of Dr Prout, regarding the use of the small quantities of elements found in organized tissues which are usually considered as accidental ; namely, that, by their electrical influence, (which may be most powerful, although they are present in extremely minute quantity) they may contribute to the production of those striking differences observed among bodies having essentially the same chemical combination, and whose diversity appears at first sight so mysterious. And were we disposed to speculate still further, we might imagine without much difficulty, that the chemical changes effected by the process of nutrition in the fluid contents of the capillaries, might give rise to a new set of electric attractions and repulsions, by which the independent circulation in these vessels might be maintained. If this speculation should prove true (and Sir J. Herschel has expressed his belief, that the circulation of the fluid in chara is analogous in its immediate cause to the motion of globules of mercury or an amalgam submitted to the poles of a voltaic battery) there will be no necessity to have recourse to the theory of a distinct set of *vital attractions and repulsions*, which otherwise is necessary to account for the phenomena. It can scarcely be regarded as a valid objection to it, that it puts out of view the influence of *mind* upon the changes in the capillaries ; since there is no more reason for supposing that mind cannot influence (through the nervous system) *electrical* attractions and repulsions, than those of a *vital* nature. In both cases the mind acts upon the properties of matter ; and the great obscurity lies in the transition from the immaterial to the material form of existence.

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* Alison's Physiology, p. 58.

28. We now come to consider those essentially *vital* properties to which nothing analogous is presented in the inorganic world, which are only manifested by organized structures, and whose existence necessarily involves the idea of their continued communication from parent to offspring.

The writer of this essay considers that we are fully warranted by evidence in rejecting the supposition of spontaneous generation as commonly understood, that is, the origination of organized beings by fortuitous combinations of inorganic matter. He thinks, however, that there is also evidence to render it a question worthy of consideration, whether plants or animals of a low degree of organization may not be developed by degenerations of the tissue of those more elevated in the scale. Our belief that such should *not* be the case is founded only on a limited experience, including only the highest classes in each kingdom. There is no anatomical difference, as far as our means of observation extend, between such a simple cellular plant as the *protococcus*, and a single vesicle from the parenchyma of a vascular plant ; and recent observations on the development of the lower cryptogamia have shewn that we as yet know very little of the variety of forms which the same germ may assume, according to the circumstances under which it is developed.

How far the act of organization in itself communicates to the assimilated matter the properties in question, will be a subject for future consideration; at present we shall consider (with Tiedemann) the property of assimilating, organizing, and communicating vital properties to nutritious matter, as that which most prominently characterizes living tissues. And we may go farther and say, that vital properties may exist in fluids also; for the blood must be regarded as something very different from a mechanical admixture of albumen, fibrin, saline matter, water, &c., since it manifests properties which can scarcely be considered otherwise than vital, and presents a structure which must be regarded as approaching that of organized matter. The same may be said of the descending or elaborated sap of vegetables from which the cambium is formed; since, according to the observation of Amici,* the glutinous sap of the vine, when removed from the stem, assumes, during the species of coagulation which it undergoes, regular forms closely analogous on the one hand to those of the lower confervæ, and on the other to the elementary tissues of which the fluid is the pabulum. Hence we may fully unite in the view expressed by Tiedemann, "that as the solid parts obtain from the general nutritive fluid matters which they receive into their composition and organic structure, and to which they communicate their vital properties, so also do the organs which prepare the assimilative liquids from the general nutritive fluid, appear to communicate to them, by the same act, qualities that give them the faculty of acting on the aliment so as to effect their assimilation."

Every tissue possesses vital properties peculiarly its own, besides that which we have spoken of as common to all. Thus muscular fibre is essentially characterized by its contractility, nervous matter by its capability of conveying impressions, &c. These vital properties all require the application of certain stimuli, in order that they may be excited to activity; and each property of each organ has stimuli appropriate to itself. It is usual to class under the denomination of *vital stimuli* not only those which themselves originate in vital actions taking place in the system (as the stimulus communicated by the motor nerves to muscular fibre), but also those which result from the relation of the organism to the external world. Now, if it be true (as we have already attempted to prove, § 22–27), that the actions *immediately* resulting from this relation are of a physical nature (the *conditions* only being furnished by vital processes within), we can hardly regard the physical agents light, heat, &c. as *vital* stimuli, unless we employ the term *vital actions* in its most comprehensive sense, to indicate *all* the actions going on in a living organism, which conduce to its preservation. On the other hand, the stimulus of the circulating fluid, itself possessed of vital properties (we restrict this term in plants to the descending or elaborated sap), and giving rise to actions of the most strictly vital character, cannot be regarded as itself otherwise than purely vital.

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* Annales des Sciences Nat. t. xxi.

29. Life, therefore, the sum of all the actions performed by an organized being, is, in the first place, the result of the operation of external agents upon the physical properties of the organs by which the relation between the organism and the external world is maintained. The changes which are the consequences of these actions are not identical in their effects with those which we witness in the inorganic universe, because the actions are performed under conditions furnished by the living system with an express tendency to its preservation; and the products of these actions are of a nature to supply the purely vital properties, which then come into play, with the means of their operation.

Thus the quantities of carbon, hydrogen, and oxygen, which combined, under ordinary circumstances, would form carbon and water, are united into gum and sugar (the two simplest of proximate principles) in the ascent of the crude sap up the stem of plants; these principles afford the conditions requisite for the purely vital actions, since out of them are formed, by the process of assimilation, all the complicated variety of vegetable tissues and products.

Now the manifest tendency of all these processes to a common end, has given rise to the supposition of a presiding power by which the whole are regulated—a "vital principle"— an "organic agent endowed with a faculty little short of intelligence,"

—in fact a distinct ουσια or entity, of the nature of whose existence, whether material or immaterial, modern philosophers hesitate in giving a distinct opinion. It will scarcely be worth while to examine the arguments upon which this doctrine has been maintained, since we find the highest authorities of the present day joining in condemning it as altogether unphilosophical.

"This idea is both unsupported by evidence, and useless in the explanation of facts."—Alison's Physiology, p. 3. "The hypothesis of a vital principle is a weapon ready to cut any knot, but capable of untying none."—Prichard on the Vital Principle, p. 128. "It has been proved by a careful examination to be wanting in every characteristic of a legitimate theory."—Ibid. p. 132. "Ainsi, les forces premières, que l'on dit animer les divers corps naturels, ne sont pas des êtres réels, existants par eux-memes, comme on le croyait dans l'ancienne philosophie."—Adelon, Physiologie, tom. iv.

But it may not be amiss to add a few observations suggested by the foregoing reasonings, tending to shew that the supposition is not in the least degree requisite, that it is inconsistent with known facts, and that it receives not the shadow of a confirmation from the analogies of matter.

30. We have endeavoured to shew that an analysis of the functions of living beings terminates in referring them all to certain properties possessed by their component structures, which properties stand in the same relation to organized tissues as the properties of gravitation, electricity, &c., do to matter in general. Their existence must, for the present at least, be regarded as an ultimate fact in physiology. These properties are called into action by stimuli of various kinds fitted to excite them. Whatever adaptation, therefore, we discover in the various functions, both to each other and to the general end of all, implies no more than an adaptation of the structure and properties of the organs by which they are performed. Now, every tissue has its own laws of development; but all these laws are subservient to one general principle, that all organized structures shall take their origin from previously existing organisms. Our inquiry, therefore, leads us back to the first creation of each species; and if we conceive that at that period the Parent of all impressed upon the elements of which each was composed, the spirit of the laws which should in future govern its growth and reproduction (just as he impressed upon the

bodies composing the solar system the spirit of the laws of gravitation and of motion, § 8), we require nothing but the continued action of these laws to account for the continued existence of the race (just as the continued action of the laws of gravitation and of motion retains the planets in their orbits). To suppose that the adaptation of these laws to each other, and to those of the external world, *could* be otherwise than perfect, would be to cast a stigma upon Infinite Wisdom. What they are, it is the object of the physiologist to ascertain by induction; and he certainly will not derive any assistance by setting out with the notion of a secondary presiding existence, whether ψυχη, *anima*, *divinæ particula auræ*, *vital principle*, or *organic agent.*

31. The doctrine of a single presiding existence controlling and directing the actions of each organism, is contrary to known facts. If such a principle exist in connection with the living body, we cannot but suppose that its departure would be coincident with what has been termed *somatic death*, that is, the general disunion of the vital functions and the cessation of the functions of the nervous system. But we well know that the vital *properties* of each one of the tissues concerned in *organic* life may be retained for a considerable period, and that *vital actions* may be performed by individual organs as long as the conditions which those actions require in the living body are supplied to them. The time at which the vital properties finally disappear varies according to circumstances; it is different in every tissue, and the extinction is not sudden but gradual. If, then, we hold the doctrine of a vital principle, we must allow that it may be split into as many individual existences as there are organs in the body; and this is, in fact, approaching to the view of the vital properties which it has been our object to establish.

32. Further, the doctrine of a vital principle is wholly unsupported by the analogies of Nature. No reflecting mind has any doubt that this earth and its inhabitants form a system (which we might almost call an *organised* one, if the idea of a particular *structure* were not involved in the term), of which every part is perfectly adapted to the rest, and of which all the actions and changes, however in appearance contrary, have one

common tendency, the ultimate happiness of the creatures of Infinite Benevolence. It cannot be regarded as an improbability, that the other spheres and systems, whose countless multitudes, revealed by the aid of science, impress our minds with the nearest approach to the conception of infinity of which our finite comprehension is capable, are peopled with beings, if not similar in structure to ourselves, at least equally worthy of the Creator's care. In the government of our own planet, itself but a point in the vast universe, we are able to recognise, to some small extent, the laws by which its physical changes are guided; and we discern faint glimmerings of those by which the moral condition of sentient beings is controlled. So far as we can understand the mutual adaptation of these laws, we every where see them working towards the same end; and we entertain the highest anticipations of the beauty and harmony which will be revealed to us when our imperfect knowledge shall be extended and corrected by the light of Eternal Truth. Should we not consider it degrading to the dignity of Infinite Wisdom to suppose, that, at the creation of each world, he had found it necessary to delegate to a subordinate the control over its working, instead of at once impressing upon its elements those simple properties, from whose mutual actions, foreseen and provided for in the laws according to which they operate, all the varieties of change which it was His intention to produce, should necessarily result. The application of this argument to the present subject is too obvious to require further development.

33. We have now to consider in what relation the vital properties of the various tissues of which a living body is composed stand towards the peculiar form in which the molecules of those tissues are arranged, and the peculiar character of the chemical combinations of which they are composed; in a single word, their organization. It appears to us that much discussion on this subject might have been spared had more attention been paid to the true meaning of the terms employed. It has been said by some that vital properties are *superadded* to organised matter. This supposes the distinct existence of the property, and is but another form of the doctrine of the vital principle. If the property be nothing but the matter itself in

a certain relation to the mind, the expression is manifestly inconsistent with itself. But there is nothing inconsistent with our knowledge of the physical properties of matter in the belief that all matter, or each at least of those forms of it capable of being assimilated, is also endowed with vital properties which remain undisplayed or occult (see § 5) until brought into action by being subjected to those conditions which a living organised system alone can afford.

Experience and observation lead to conclusions not dissimilar. Organization and vital properties are simultaneously communicated to the germ by the structures of its parent; those vital properties confer upon it the means of itself assimilating, and thereby organizing and endowing with vitality, the materials supplied by the inorganic world; and, as long as each tissue retains its normal constitution, renovated by the actions of absorption and deposition by which that constitution is preserved, and surrounded by those concurrent conditions which a living system affords, so long, have we every reason to believe will it retain its vital properties and no longer; and, just as we have no evidence of the existence of vital properties in any other form of matter than that denominated organized, so have we no reason to believe that organized matter can exist in those conditions which a living body supplies, without manifesting vital properties.

There is no difficulty in accounting, on this view, for the general loss of vitality which results from the cessation of any one function; and as the advance of pathological science renders it every day more probable that derangement in function always implies some structural alteration (although it may be imperceptible to our senses) or change in the character of the necessary stimuli by which the properties are called into action, the usual phenomena of death can readily be explained upon the principles here advocated. There are some cases of sudden destruction of vitality, however, in which no perceptible change in solids or fluids can be detected; but this is probably owing to the imperfection of our means of research. For instance, we can scarcely doubt, that, when the vitality of an egg is destroyed by an electric shock, or moderate exposure to heat, the agent produces, in obedience to chemical laws, some alteration in the material structure inconsistent with the continued existence of the vital properties. A seed may be deprived of its vitality by immersion in water of the temperature of 160°, and nothing but a very minute examination would discover any structural alteration in its tissues; but collateral experiments prove that this is just the temperature at which the vesicles of fecula are ruptured, so that the physical change produced by a physical agent is evidently the cause of the loss of vital properties.

34. Our general conclusions, then, from the foregoing arguments, are,—

1. That the *properties* of any aggregation of matter depend upon the method in which its ultimate molecules are combined and arranged.

2. That the simplicity of our notion of the properties of inorganic matter depends upon the facility of our becoming acquainted with them through the command which we possess over the agencies by whose operation they are manifested.

3. That the vital properties of organized tissues are not less the result of their material constitution; but that, whilst the *materials* of an organized tissue may be prepared by the operation of the ordinary laws of affinity acting under peculiar conditions, the tissue cannot be constructed without the agency of a previously existing vitality; and that hence man is debarred from the most advantageous means of becoming acquainted with the laws of physiology.

4. That *vital properties* are not *added* to matter in the process of organization; but those previously existing, and hitherto inactive, are called out or developed.

Observations on Rabies or Madness in Dogs, Oxen, Horses, Pigs, and Sheep. By Dr WAGNER, Medico-Forensic Censor of the Schieben District.

THE following observations are interesting and valuable, on account of their authenticity. We particularly recommend them to the notice of gentlemen practising in the country, who may have opportunities of witnessing similar cases. We are firmly convinced that medicine is destined to derive no slight advantages from observations made on the diseases of animals; and, on this account, as well as others, we are gratified in observing the gradual, but still too slow, improvement in the pathological knowledge of our veterinary surgeons.

1. *In the Dog.*—A dread of water is not a symptom invari-

ably attending rabies. I have met with two instances of men being bitten by dogs, and dying from rabies, when the dogs, nevertheless, ate and drank shortly after having made the attack. One of these was seen to swim through the river Elster on the ensuing day. Another, in the same state, displayed great activity in the water for several hours, in search of wild fowl, until, on a sudden, it ceased to recognise its master, appeared to have lost the senses of sight and hearing, rushed out of the water, and scampered away, snapping on the road at every object within its reach.

Though rabid dogs are frequently in the habit of gnawing at wood, straw, or hides, snapping round about in the air, &c., such signs are equally fallacious with the foregoing one, healthy dogs being sometimes observed to do the same.

An appearance of dejection and shyness, a hanging of the head and tail, running at the eyes, or the fierce flashing eye without this, foaming at the mouth, refusal of food and drink, are all suspicious symptoms, and demand caution; but as they are attendant upon other canine diseases likewise, they are not conclusive. In fact, experience has furnished me with no evidence *decisive* of the existence of rabies, except that of a dog running away and recklessly attacking both men and animals, especially those of its own species. No other dog, even the most vicious, when accidentally separated from its master, will attack any save animals of its own kind, unless in self-defence. In a rabid state, some dogs will even leap over high fences, in order to reach dogs or cats which they discover to be on the other side; a proof that scent, sight, and hearing are still unimpaired. Others will sneak along a wall, or run forward in a straight line, attempting to bite whatever they thus meet with, but never diverging from their headlong course, unless to pursue other dogs. I have observed some rabid dogs which never moved from one spot, merely gnawing at every thing within reach, as they lay, snapping at vacancy, and ultimately refusing food and drink, or, if they attempted to partake of either, appearing incapable of deglutition. In those cases, palsy of the loins and hind extremities appears to have taken place at the outset, and to have deprived the animals of the power of locomotion. Drooping of the tail and head, and foaming at the mouth, are, of all signs, the least to be relied on: they belong only to the last stages of the disorder, when the animal has almost ceased to live. On the other hand, it is a well known fact that some dogs, in a state of health, constantly foam at the mouth, and that all look dejected and hang their tails when they have wandered astray.

The same observation applies to the observation of barking, or the hoarse bark. Many dogs bark but rarely, whilst with others the bark is perpetually hoarse; on the contrary, many rabid dogs continue to growl or raise a howling bark (so that a single short bark is succeeded by a shrill lengthened howl), similar to that made by healthy dogs at certain sights or sounds, from hunger, thirst, &c. Even in the appearance of the eye in rabies, the unprejudiced observer will not discover any essential change.

The moment a dog evinces any traces of illness, it is no longer to be trusted; and it would be well to lock it up, or fasten it to a stout chain. But when it begins to gnaw wood, to shew a dull eye, to snap at animals with which it had become familiarized, to bark hoarsely; when it attempts to run away or to break its chain, eats and drinks with a snapping gesture, at intervals appears lively, and then again sneaks sulkily to its kennel; when it disregards its master's call, and, contrary to its former habits, growls and snarls at well-known persons; the animal ought to be despatched; for there can no longer remain a doubt of its being rabid. If a dog runs away, and returns home on the second or third day, with any unhealthy symptoms, its rabidness is equally certain, more particularly if it then ceases to know its master.

The rabid dog is often very sudden in its motions, darting at its victim with the quickness of lightning. In the first stages it bites slily, and rather pinches than wounds; but, at a later period, the bite is so terrible, that force is sometimes necessary to disengage the animal. I witnessed two cases in which the dogs could not be made to relax their hold until their spine had been divided by a blow with a hatchet.

2. *In Oxen.*—In horned cattle, amongst which I have met with the most numerous instances of rabies, this disorder evi-

dently occurs under two distinct forms. In solitary cases, where we are previously aware of the animal having been bitten, we should scarcely be led to a suspicion of rabies. The commencement and progress are as follows:—With a healthy exterior and clear eye, the animal loses its appetite, eats and drinks by fits only, without, however, refusing choice morsels that may be offered to it; at times it appears as if suddenly stupified, recedes from the manger, and forgets to chew, then again chews on, listens with liveliness to every noise that is made, seems to notice every object, continues to obey its keeper, and sometimes remains capable of working: borborygmi are, however, already to be heard, and there is sometimes observable a slight straining or disposition to tenesmus. In the cow, the secretion of milk decreases, but by no means ceases at the very commencement. In the open air, the animal ceases to graze, appears lost, strays from the rest of the herd; although when, by itself, it generally allows itself to be led quietly back to the stable.

After the lapse of from one to three days, all the symptoms increase; neither hunger nor thirst is experienced, although no dread of water is apparent, and the animal still ruminates at intervals. The eyes have commonly a healthy look, but flash at times without seeming inflamed. The creature lows but seldom; but when it does several times consecutively, and either with a hoarse tone, or, on the contrary, in so clear and powerful a key, that I have heard it distinctly at a distance of more than a mile. The mouth and tongue have still nothing abnormal about them. The borborygmi increase. At the same time the animals are observed to lick some particular part of their bodies, especially one of the feet (probably the spot where they were bitten) until that part becomes excoriated and bleeds. Palsy of the loins now ensues; the animal remains lying, and, if forced to rise, which it does with difficulty, totters on its hind legs; an increased straining is remarked at the anus, and this is sometimes followed by hard but ultimately by thin evacuations; it moves its head alternately from side to side, and appears anxious to lick at cloth or fur, &c., which it by degrees gets between its teeth, and tears. To this the suffering animal appears not to be urged by any vicious propensity. At this period, in the cow, the secretion of milk is at an end.

The above symptoms continue on the increase until the sixth or seventh day. At length the hind extremities become so paralysed as to render standing on the feet impossible; the constant straining at the rectum still forces out fæces, and the animal refuses all, or almost all, further sustenance; even bread that is forced with the hand into the creature's throat, is not swallowed. A dread of water is never observed, even though the latter be refused. Naturally thin and rough haired cattle become still thinner, and their hair rougher; but such as were originally in good condition and smooth-haired, shew little alteration in either respect. Some time between the sixth and the ninth day, the animals sink on one side (mostly, I have observed, on the left) with the head commonly stretched backwards; the eye still remaining bright, lively, and uninflamed. From henceforth the trunk continues motionless, whilst the legs undergo a constant but languid movement to and fro, until the animal has ceased to exist. In the form above described, the animals may be approached with little or no danger. Not so in the second form; of which, however, scarcely one case is observed in six.

This form commences like the other. In the stable, the animals recede still more from the manger, and will even break the rope with which they are fastened, if it is not very strong. Their bellowing, without being frequent, is prolonged, its tone clear, thrilling, and unmodulated. They scratch the ground with their fore-hoofs, often with such violence, that the dung is projected to the roof of the stable; whilst with their hind legs they kick violently at any one who approaches. At this stage, the paroxysms are periodical, and little is to be observed during the intervals, except a reluctance to feed or drink. About the fourth day, if they are naturally strong, they will snap every kind of fastening in twain during the paroxysm, and then attack and gore all who approach them. They rage about in the stable, gnawing the ribs and other objects to pieces, until palsy of the joints supervenes, when they sink down, and ultimately fall on one side. In this position they still manage, spasmodically as it were, to shove themselves about the stable by means of their hind legs. Last of all, they lie for hours on the side, as if dead, when the bright shining eye, and occasionally a convulsive movement, alone indicate a remnant of life. On the seventh, eighth, or at most the ninth day they die.

On dissection, nothing abnormal is to be discovered beyond a gall-bladder filled to excess with muddy yellowish-green coloured bile, in a state of complete fermentation. From the great distention of the bladder, the fermentatory process is easily distinguished from without.

3. *In Horses.*—With horses the symptoms of rabies are as follows: In the team they display unwonted activity and emulation. Here, however, as well as in the stable, they assail their fellows with bites and kicks. They generally refuse both provender and water, nod or jerk the head frequently, (as horses are in the habit of doing, when they are in spirits), and have a fiery eye. On the second, or at most on the third day, these symptoms increase to such a degree, that no creature can approach them without being bit and kicked in the most frightful manner, and to offer them either food or water appears only to augment their rage. Yet moments of quietness intervene, provided the stable be kept dark and nothing around them stir. On the other hand, such is their fury when disturbed, that on a plank being taken out of the stable door, in order to destroy them with safety, they will rush to the spot, and thrusting their head through the aperture, make attempts to bite in every direction. How long such a state continues, until death occurs spontaneously, I am not able to determine, as in the cases which came under my observation, the natural termination was not waited for.

4. *In Pigs.*—During my long continuance in office, I met with but a solitary instance of rabies in the pig. This animal, afattening boar, had been bitten, four days previously to my seeing it, by a mad dog, but it already raged with such violence, that I could only observe it through the crevices of the sty wherein it was inclosed. It gnashed its grinders, jumped up at the sides of the sty at one instant, and threatened to burst through them at the next, by the violence of its efforts. I di-

rected it to be killed, and, together with the cleansings from the sty, to be buried at a great depth in the ground; which I was afterwards assured had been done. I subsequently discovered, however, that a butcher who had slain the animal with a hatchet, instead of allowing the carcass to be buried, had cut it up, and exposed it for sale, and that, from its fatness, it had been readily disposed of. Notwithstanding the infamy of this act, aware that the possible evil consequences could no longer be averted, I too much dreaded the effects of the imagination in those who had partaken of the meat, to hazard a legal investigation on the subject. The disgraceful occurrence therefore slept in silence; but from that day to this, and it happened twenty-four years ago, I have not heard of any mischief arising from it.

5. *In Sheep.*—In like manner, I have met with but one instance of rabies in sheep; in this one, however, five sheep, which were bit by a mad dog, all became affected at one and the same period, after the lapse of a few weeks. The symptoms first noticed were, that the animals left off grazing, and dispersed the flock by indiscriminate attempts to butt and to mount upon their fellows. They were all killed save one, which being confined in a stall, kept quietly staring at the wall with a dejected look, as long as every thing around it was noiseless; but on hearing the slightest sound, the animal turned to the direction from whence it proceeded, and jumped up at the sides of the stall. Food and water were left untouched. Here my observations were cut short by the proprietor declining to await the natural termination. In a large flock, single cases of disease are usually overlooked until the symptoms become very conspicuous, and it appears more than probable that such was the fact in the present instance.

The above details are therefore only to be received as a rough sketch. The peasants would frequently hold fast the tongue of the rabid cattle with one hand, whilst they thrust the other deep into the creature's throat, in the endeavour to force nourishment into its stomach. That the naked hands and arms were on some of these occasions not entirely free from injury, I can scarcely doubt; and yet I never witnessed any evil effects to arise from the practice, although these people neglected to wash their hands subsequently. However, I knew a farmer to die of rabies, merely from having, with hands apparently uninjured, washed out the wound inflicted on a pig by the bite of a rabid dog. On one occasion, a slaughterer who had killed a rabid ox, in spite of my admonition, repeatedly thrust his naked arm into the animal's intestines, without experiencing any evil therefrom, and cow-herds, *soi-disant* veterinary surgeons, and the like, often commit similar acts with equal impunity.

I have frequently known the milk of rabid animals to be taken without detriment, and in two instances, the flesh of rabid oxen which were clandestinely eaten, proved no less innoxious. It is nevertheless a fact, that, at a period when many cattle perished of rabies, the instances of canine madness became unusually numerous ; and it may be supposed that, however deeply the carcasses may be buried, dogs will still here and there succeed in disinterring them. Farther experience is therefore requisite to decide on the effects of the saliva, blood, excrements, &c. in all domestic animals, with the exception of the dog and the cat. In conclusion, I have witnessed many instances where the bite of decidedly rabid animals has produced little or no effect on the human subject, although the remedies employed were merely such as were suggested by superstition. From this I am led to infer, that with mankind a predisposition to hydrophobia very rarely exists.*

Analysis of Gmelinite or Hydrolite. By A. Connell, Esq., F.R.S.E., &c. Communicated by the Author.

The crystallographic and optical properties of this mineral were described a few years ago by Sir David Brewster,† who considered them sufficiently characteristic to entitle it to be ranked as a distinct species. Two varieties of it had been analysed some time previously by Vauquelin, with the following results[*] :—

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* Hecker's Annalen, 1st Band, Heft 4. 1836 ; and Forbes and Conolly's Medical Rev. No. ix., p. 262.

† Ed. Jour. Science, ii. 262.

	Montecchio Maggiore.	Castel.
Silica,	50.00	50.00
Alumina,	20.00	20.00
Lime,	4.5	4.25
Soda,	4.5	4.25
Water,	21.	20.
	100.	98.50

Some time ago Mr Rose, mineral dealer of this city, was so good as to give me a very fine specimen of Gmelinite from the county of Antrim in Ireland, for the purpose of being subjected to a chemical examination.[†]

The specimen was in fine crystals of considerable size, and possessing the usual form of a very short six-sided prism terminated at both extremities by a six-sided pyramid. The truncation of the apex was only occasionally observable. The crystals were of great purity, being white and semitransparent, whilst in many Irish specimens they are reddish, and scarcely translucent. The prism was streaked horizontally, and the pyramid parallel to its own terminal edges, in the manner shewn by Haidinger's figure of the crystal,[‡] although less distinctly. It may be proper to notice, that I could scarcely observe the quality stated by Sir David Brewster, of bristling up and scaling off when approached to the flame of a candle; although I noticed it in a reddish and somewhat opaque specimen of Irish gmelinite.

(*a*) 17.67 grains of the crystals reduced to impalpable powder, were decomposed by muriatic acid. The silica separated as usual, weighed, after ignition, 8.97 grains. It was dissolved in boiling potash ley, and left a little imperfectly decomposed mineral, from the silica of which, .45 of alumina and .059 of lime were separated by fusion with carbonate of soda and other necessary steps. The proper amount of silica was thus 8.461 grains.

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[*] Ed. Jour. Science, ii. 262.

[†] The Irish variety of this mineral was analysed, a few years ago, by Dr Thomson, but the quantity operated on was only 5.3 grains, which were decomposed by carbonate of barytes; and the result differed considerably from that of Vauquelin. Trans. Roy. Soc. Ed. xi. 448.

[‡] Mohs' Mineralogy, fig. 195.

(*b*) From the aqueous solution of the muriates of the bases, ammonia threw down a precipitate which, after ignition, weighed 2.85 grains. When this was dissolved in muriatic acid .09 of silica remained undissolved, and by boiling the solution with excess of caustic potash, .02 of oxide of iron, containing a little lime, were obtained; thus leaving 2.74 grains of alumina.

(*c*) By means of oxalate of ammonia and moderate calcination of the precipitate, 1.82 of carbonate of lime were obtained, equivalent to 1.025 of lime.

(*d*) The remaining solution yielded, by evaporation and ignition, a residue of 1.42, which, when dissolved in water, left .03 of silica, and yielded cubical crystals by evaporation; and when redissolved, a precipitate was obtained with muriate of platinum, and by evaporation, abundance of crystals of the double chloride of sodium and platinum. These last were taken up by heated spirit of wine, leaving .36 of the double potassium salt, equivalent to .069 of potash and .109 of chloride of potassium. There thus remained 1.281 of chloride of sodium, equivalent to .682 of soda.

In 17.67 grains of the mineral, there are thus contained, exclusive of water,

Silica (*a b* and *d*)	8.581
Alumina (*a b*)	3.19
Lime (*a c*) .	1.084
Soda (*d*) .	.682
Potash (*d*) .	.069
Oxide of Iron (*b*)	.02
	13.626

To determine the quantity of water, a portion of the crystals was ignited in a platinum crucible and charcoal fire, when he loss of weight amounted to 21.66 per cent.

We have thus, in 100 parts of the mineral:

		Oxygen contained.	
Silica, .	48.56	25.226	9
Alumina, .	18.05	8.430	3
Lime, .	6.13	1.721	1
Soda, .	3.85	.984	
Potash, .	.39	.066	
Oxide of Iron,	.11		
Water, .	21.66	19.253	7
	98.75		

It sufficiently appears, both from the analyses of Vauquelin and from that here detailed, that this mineral is nearly allied to chabasite, in a chemical point of view, as, according to Mr Haidinger, it is crystallographically; and it is not impossible, that if analyses applicable to different localities were repeated and sufficiently extended, the chemical formula for chabasite might be found to embrace gmelinite. It does not, however, apply to the above analysis, and still less to those of Vauquelin. The formula indicated by the above result is, $(C\,N\,K)\,S^3 + 3\,Al\,S^2 + 7\,Aq.$; and it may be noticed that whilst this formula exhibits one atom of silica and one atom of water *more* than that for chabasite, the formula which a former analysis of levyne, a mineral also nearly allied to chabasite in a chemical view, suggested, shewed one atom of silica and one atom of water *less* than in chabasite;* and that in gmelinite, bisilicate of alumina is associated with tersilicate of lime and alkalies; in chabasite, with bisilicate; and in levyne with silicate of these bases; as appears from the formulæ:

$(CNK)\,S^3 + 3\,Al\,S^2 + 7\,Aq.$ Gmelinite.
$(CNK)\,S^2 + 3\,Al\,S^2 + 6\,Aq.$ Chabasite.
$(CNK)\,S + 3\,Al\,S^2 + 5\,Aq.$ Levyne.

I have much less expectation that the chabasite formula will ever be found to embrace levyne; because the proportion of silica and that of alumina, actually found in the latter mineral, differ in a marked manner and in opposite directions from those in chabasite; whilst in gmelinite the difference is much less considerable, although still excluding the chabasite formula.†

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* London and Edin. Jour. v. 40.

† It was objected, on high authority, to the formula for levyne, that the alkaline bases cannot be in a lower state of saturation than the earthy, because they will, by their stronger affinity, take silica from the latter. But might not a similar argument be employed against their ever being in *an equal* state of saturation; and may it not be asked, how, on this principle, an alkaline base should ever rest satisfied until it had at least got into that state of saturation which the same high authority considers to be the *neutral* state? It does not seem very unreasonable to suppose, that the relative states of saturation may be influenced by the relative quantities of acid and bases present.

Upon Glaciers, Moraines, and Erratic Blocks; being the Address delivered at the opening of the Helvetic Natural History Society, at Neuchatel, on the 24th of July 1837, *by its President*, M. L. Agassiz.

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Gentlemen,—It is now a considerable period since the Neuchatel members of our Society have longed for the time when they could invite their associates throughout Switzerland, to congregate amongst them. Circumstances, however, over which they had no control, and more especially the erection of the building in which we are now assembled, and which is intended for the reception of the various scientific collections, have compelled them to delay this honour till they could proffer it without inconvenience to their guests, and till they could submit to their inspection a part at least of their museum. Even now, in spite of the extraordinary exertions of the indefatigable keeper of our museum, but a small portion of the specimens are yet displayed; they have likewise been arranged with haste; and the workmen have scarcely quitted the premises. We therefore need your indulgence, but trust you will not the less doubt the satisfaction with which we receive you, and the high gratification it affords us. From our hearts we gladly bid you welcome!

On such an occasion as the present, we are naturally led to inquire, What is the bond which connects the different sciences that are prosecuted by our Society? I do not hesitate to affirm that one grand idea pervades all the exertions directed to the extension of the limits of these sciences;—it is a belief of a progressive development throughout creation, of a transformation extending through different states which are dependent upon each other;—it is the conception of an intelligible creation, concerning which our task is to ascertain the relations in all the phenomena which come under our observation. In this view we may regard Astronomy, engaged with the formation of the celestial spheres; Chemistry, investigating the different actions which bodies exert upon each other; Natural Philosophy, appreciating the nature of those forces whose actions are ascertained; Natural History, considering the phases of life throughout animated nature; and, finally, Geology, which ventures to comprehend the history of our globe, to decipher its oldest records, and to represent them as one grand whole, whose various revolutions have invariably tended to one great result.

From the progress of these, there will, no doubt, one day appear a noble result, something worthy of *Man*, which will introduce the cultivation of the natural sciences within the circle and sympathies of social life, more powerfully than all the advantages which they bestow upon industry and the arts, however prodigious these may be.

Our society has not been a stranger to the movement here alluded to, and the names of its members appear honourably by the side of those masters in science who have united with us in our labours. This very day's meeting, better perhaps than any other, may prove that my assertion is not at all exaggerated. You, gentlemen, know that our small society has served as a model to those vast associations in Germany, England, and France, which can boast of so many illustrious names; and if the labours which it has undertaken have appeared less brilliant than those accomplished by greater societies, it has at least given the first impulse to more than one interesting undertaking.

Still more recently, two of our members have, by their researches, given rise to discussions of the deepest importance, and the result of which will resound far and near. The locality in which we have now met, leads me again to introduce to your notice a subject whose solution will be found in the examination of the slopes of our Jura. I mean to speak of glaciers, moraines, and erratic blocks.

Every one in Switzerland is familiar with the glaciers, and knows that their margins are bounded by dykes of roundish blocks which are called *moraines*, and which are continually pushed forwards, or abandoned, by the glaciers, as these advance or retire. The inhabitants of the Jura especially are acquainted with another phenomenon which is striking among our mountains,—I mean that of *erratic blocks*, or those masses of granite and other primitive rocks, which are found chiefly on the southern slopes of the Jura. It is not, however, so generally known that other moraines exist besides those which, at the present day, skirt the edges of the glaciers. Messrs Venetz and Charpentier were the first to introduce these to notice, and they have observed them chiefly in the lower valleys of the Alps. One point regarding these remains still to be discussed; it is the connexion which it has been endeavoured to establish between these erratic blocks, and the glaciers which formed the nucleus or matrix, so to speak, of the great moraines, the traces of which are still found upon the northern shores of the Lake of Geneva. It is to this point I now intend to request your attention.

The *facts*, at all events, which have been observed by Messrs Venetz and Charpentier* have been gained for science; and it is also important to proclaim their extreme accuracy, for upon this naturally depends the validity of whatever conclusions may be legitimately deduced from them.

At distances, more or less considerable, from the existing glaciers, we find, at different elevations, moraines which are perfectly similar to those which still encircle the glaciers. They are equally concentric, and form walls which follow the sinuosities of the sides of the valleys. Every where many stages of them may be discovered, the most elevated of which may be found some hundreds of feet above the bottom of the upper valleys of the Alps, where glaciers now no longer exist. In descending into the *lower* valleys, we still encounter them, at the successive elevations of twelve and fifteen hundred feet, and even at eighteen hundred feet; there are also some, which are quite distinct, at a height of two thousand feet above the bed of the Rhone, in the neighbourhood of Saint-Maurice in the Valais. They may be followed even to the margin of the Lake of Geneva. Some very elevated ones exist above Vevey, and in the environs of Lausanne, which correspond with those on the southern side of the lake.

If these moraines are not generally noticed, this is owing to the fact, that they are much more elevated than are any of the common routes, and that those occurring in the lower districts have generally been much disturbed by descending torrents.

It is not at all a difficult matter to distinguish these ancient moraines from the dykes which are formed by the overflow of water, and from the masses of debris, more or less extensive, which are produced by avalanches. The dykes are very irregular, and extend only to small distances, when they gradually become level; the masses of debris from avalanches again assume the shape of very flattened cones, debouching from the valleys, and gradually losing themselves in the plains; whilst the moraines are continuous and parallel dykes or walls, which extend along the two sides of the valleys, and are formed of rounded blocks, that have been evidently triturated, so to speak, *in situ*, one against the other, as actually happens at the margin of the modern glaciers, which occur in long and narrow valleys. The blocks of avalanches, on the contrary, are angular; those of the dykes formed by torrents may, indeed be rounded, when, for example, they are derived from broken up moraines, but then they extend themselves in irregular plane surfaces or sheets; and when they are derived from recent avalanches, they are equally angular, unless in their progress they meet with ancient moraines, which they carry along with them, and mix with their own constituent parts.

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* See this Journal, vol. xxi. p. 210, and vol. xxii. p. 27.

That we may be convinced of the accuracy of these facts, all that is necessary is to traverse the valley of Chamouni, there tracing the moraines which approach nearest to the glaciers, or to mount perpendicularly upon the sides of the valley of the Rhone, between Saint-Maurice and Martigny, on the left bank, above the Pissevache, near the hamlet of Chaux-Fleurie (Tsaufria), or on the opposite bank, ascending from the village of Morcles to the baths at Lavey. The rubbish of the late *debacles* of the Dent du Midi, the immense avalanches whose traces are every where seen, and the numerous dykes formed by the Rhone, will, moreover, enable us justly to appreciate the difference which exists in those various phenomena produced by such different causes.

Lateral valleys, it should be remarked, exhibit the same phenomena, which may be seen by ascending the course of the Avençon, as far as the glacier of Paneyrossaz.

In perambulating these valleys, nothing has exited my attention more than a peculiar polished appearance which those rocks present on which glaciers have moved, an appearance which may be observed not less distinctly in all the valleys the flanks of which are crowned with ancient moraines, at whatsoever distance they may now be from the existing glaciers. In fact the whole sides of the valley of the Rhone are thus polished, to the very shores of the Lake of Geneva, more than a day's journey from the glaciers, and in all places where the rocks are so hard as to have resisted the influence of the atmosphere.

The *explanation* which M. de Charpentier has given of these facts, viz. that they are clearly produced by great masses of ice which, at a former time, had filled the bottom of all the Alpine valleys, does not appear to embrace the whole question, and the Jura exhibits a series of phenomena which lead to additional conclusions.

That I may more satisfactorily discuss what I mean to advance on this subject, I shall first make some remarks on those polished surfaces which are to be found over the whole of the southern slope of the Jura, and which are designated *laves* by our mountaineers, as we are informed by M. Leopold de Buch, the individual who, of all geologists, was the first deeply to study the Neuchatel Jura, and who has most successfully investigated the subject which now engages our attention.

The southern slope of the Jura, which fronts the Alps, exhibits these *laves* to its very summits, from the shores of the lake Bienne even to beyond Orbe, limits within which I have myself ascertained their existence.* They are polished surfaces completely independent of the stratification of the beds and of the direction of the mountain-chain of the Jura; they extend over the whole surface, following its undulations, and are equally marked upon the Neocomian and the Jurassic rocks; they penetrate into the depressions which form the little valleys, and elevate themselves on the most isolated ridges, presenting a polish not less uniform than that of a mirror, especially where the rocks have been recently exposed, that is to say, cleared of the earth, gravel, and sand which generally cover them. These surfaces are sometimes plane, sometimes undulated, and often even traversed by furrows more or less deep and tortuous, or with longitudinal very rounded elevations, but which never follow the direction of the slope of the mountain; on the contrary, like the gibbosities, these furrows are oblique and longitudinal, a direction which excludes every idea of a stream of water being the cause of these erosions. Another curious fact, which is quite inconsistent with the action of water as their cause, is, that these polished surfaces are uniform, even where the rock is composed of fragments of different degrees of hardness, and the shells which it contains are sliced as in an artificially polished slab of marble. We likewise remark, upon the surfaces which are at all fresh, a number of fine lines or scratches, similar to those produced by a diamond on glass, and which in general follow the direction of the oblique furrows.

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* They extend much further, as we learn by a letter from M. Schimper, received July 25, and inserted in the Transactions of the Society, p. 38.

The localities in which these appearances may most readily be perceived in the environs of Neuchatel, are the Mail, on the side of the lake, at the surface of the *Neocomian* formation, and at the Plan, at the spot where the old road joins the new one. The most remarkable, however, are at some distance from the town, as, for example, above Landeron, on the surface of the *Portland* rock, at the confines of the vineyard and the forest, in the neighbourhood of Saint-Aubin and above Concise.

In some localities great excavations may be observed, and even something very like walls, which could only have been produced by the cascades which descend between the fissures of the ice. To any one who has examined in the Alps the bottoms of the ancient glaciers, it appears manifest that it is the ice which has produced these polished surfaces, as, for example, those in the valley of the Rhone, to which we have above alluded. It is worthy of remark, that these surfaces are never found at the bottoms of the small longitudinal valleys which are formed by the abrupt faces of the different zones of beds which compose our chains, nor even upon the escarpment of those of such mural walls as look towards the mountain, whilst I have noticed them on many steeps which look towards the Alps, as, for example, along the new road between Saint-Aubin and the Castle of Vauxmarcus.

It is of no less importance to point out the differences which exist between these *laves* and the other polished surfaces with which they may be confounded, but which resemble them only in a few particulars. I allude to the polished surfaces which are produced by faults, or by the sliding of strata upon one another. The former of these, penetrating vertically or obliquely across many strata, are not visible except when one of the sides of the ruptured rock is sunk; they are, moreover, never seen over a somewhat extended surface, as are the laves: the latter, or sliding strata, sometimes present surfaces which are pretty extensive, when the beds above the slide have been removed; but then the grooves or furrows produced by the sliding are in the direction of the slope, which is by no means one of their characters on the surface of the *laves*. Surfaces which are polished by the action of water also possess a peculiar character, whether produced by running streams or by more considerable masses of water contained in a lake, &c. In the former case the sinuous furrows always descend; whilst the furrows and the gibbosities of the *laves* ascend and descend with the accidental surface of the polished rock. In the latter case, the waters, agitated near the shore by the winds, and raised above their habitual level, always regain their equilibrium, and thus form unequal furrows, more or less deep, which usually follow the line of the greatest inclination, at all events when there are no local peculiarities to impart to them a different direction. This occurs when the waters of the lake are high or low, in spring-time and autumn. All these differences may be studied in the immediate neighbourhood of this city, by comparing the polished surfaces of the *Mail* with the erosions produced by the lake in the prolongation of the same beds, or with the sinuosities which have been produced by the Seyon in its ravines. I may add, that the polished surfaces produced by the action of water are never so smooth as these *laves*, or as the surfaces polished by the glaciers. When the water is charged with sand or mud, the same effects are produced, with this modification, that they are not quite so marked. I have not hitherto had occasion particularly to study the effects produced by great masses of water charged with ice; I believe, however, they are much the same as those produced by simple water. One thing is clear, viz. that in the beds of our rivers, and at the margins of our lakes, both of these effects are produced; and it is moreover evident, that floating ice can produce no action at the under surface of the water that supports it. It follows, therefore, that it is only the movement of great masses of ice immediately upon solid masses, which can produce effects similar to the polish which we remark upon the margins of the retreating glaciers. This last phenomenon perfectly resembles that which is exhibited by the *laves* of the Jura.

By this similarity alone we might be led, in application to this phenomenon, to conclude, that like causes have produced like effects. But there is another class of considerations which more directly lead us to associate these two phenomena, and which will compel even those who would resort to different agencies, to regard them under one and the same point of view.

We have witnessed moraines on the very margin of the lake of Geneva, and on both banks, at the same elevation; we have thereby the certainty that there was a time when the lake was frozen to the bottom, and when this ice was elevated to a considerable height above its present level.

But we also know that all the moraines which remain *in situ* are such as the glaciers leave in retiring. Since the epoch, then, which I have just mentioned, or in which the glaciers still debouched in the lower Swiss valleys, they have gone on diminishing and retiring into the more elevated and higher valleys.

Here, then, a question naturally suggests itself, Have those glaciers which have extended to the greatest distances, descended from the summit of the Alps? or was there a time when the ice was naturally formed beyond the limits within which we now see it confined, reaching even to the Jura, and possibly beyond it?

The level of the moraines on the shores of the Lake of Geneva, viz. 2500 feet above the sea, and the nature of the polished surfaces on the Jura, would lead to this conclusion. We have, moreover, only to mark on a chart of the levels the heights of the moraines which debouch in the different localities of the alpine chains, to be convinced that ice at one time covered the whole of the plain of Switzerland, and reached the slope of the Jura. *

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* M. Rod. Blanchet, who has been engaged upon this subject, has some

In truth, the difference of level between the elevation of the moraines on the border of the lake of Geneva, of those in the neighbourhood of Vevey, and on the side of Savoy, and that of the polished surfaces which are observed above the margin of the lake of Neuchatel, and onwards to the very summit of Chaumont, is such that the sheet of ice which filled the space comprised within these limits, must have had a certain inclination, since the level of the lake of Neuchatel is only 1344 feet above the level of the sea, that of the zone of Pierre-à-Bot, along which the greatest number of boulders is found, is 2150 feet, and the summit of Chaumont itself is only 3619 feet above the level of the sea.

This being the case then, we are not only entitled to attribute to the action of ice all the polished surfaces we find on the slope of the Jura, but also to regard these surfaces as a certain index of the wider limits which the ice had at a former period both over the Jura and the Alps.

M. Charpentier imagines, that these masses of ice were glaciers which were formed on the summit of the Alps, and which had descended into the plain, and had then been elevated to the heights in which we now find their traces, forcing before them the blocks which are now reposing on the Jura. There is a striking fact, however, which is in opposition to this explanation: it is, that the Jura blocks are usually less rounded and even of a larger size than those which are found in the moraines at the margin of the existing glaciers.* If our blocks had been thus rolled in front of a glacier from the Alps as far as the Jura,

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time ago remarked, that the summit of the Pèlerin, the mountain which overhangs Vevey, fronting the opening into the Vallais, and elevated 3301 French feet above the sea, and which is composed of a coarse-grained conglomerate, is polished near the top, at a spot where there is no water even to form a little rivulet, and no path, or any other polishing cause which can be adduced. It is therefore to a height of 3300 feet at least, that we may carry the elevation of that ice which filled the basin of the Lake of Geneva, the surface of which lake is now at an elevation of 1145 feet. On the summit of the Pèlerin is the *resting-place* of the ice whose level was 3300 feet above the level of the sea. We have no data, however, to indicate what was its thickness at this place.

* These facts do not at all agree with what M. Elie de Beaumont has described respecting the valley of the Durance.

they must generally have been rounded and smaller, and there must also have been immense moraines reposing on the Jura, which, however, have no existence.*

The prevailing opinion attributes the transport of these blocks to vast currents of water, or to floating ice.

The greatest difficulties which exist in this theory, and we shall mention but a few of them, are first to give a sufficient explanation of the origin of these currents, and of the rapidity it is necessary to assign them, in order that they might be able to transport such enormous masses; if, at the same time, it be admitted, as every thing seems to indicate, that they have been transported *after* the elevation of the Alps. For in this case these currents must have started from the *crests* which separate the valleys, because the phenomenon of the blocks presents itself in all the Alpine valleys, and on both slopes of the chain; that is to say, to meet the exigencies of the case they must have been projected from all these crests† with an impetuosity which would not permit the blocks to fall below the level in which they are found in the Jura and the Alpine valleys where now there are no glaciers, for even the existence of the great moraines is denied, in order that the deposition of their blocks may be attributed to the same streams. But how is it possible that currents of water of several leagues in length (I here speak of the lateral valleys which debouch into the principal ones) could have conveyed such immense blocks to an elevation of more than a thousand feet? Moreover, the fact that the blocks of the different valleys are not similar, and that they are spread, fan-shaped, to a certain distance round the Alps, excludes the idea of that extreme rapidity which has been ascribed to the currents for the simple purpose of explaining this transport, without considering that it must, at the same time, have produced effects, a trace of which is no where to be found. This fact, then, most completely excludes the idea of *a grand diluvial current* passing over all Switzerland in any direction whatsoever. If, again, it were before the elevation of the Alps it is supposed the phenomenon occurred, I ask, how has it happened that the lines of these blocks formed in the Alps have not been dislocated by the uprising? For in this case the continuous and parallel dykes of blocks which may be seen *upon both flanks* of the whole of the Alpine valleys, and which follow them in all their windings, whatever may be their direction and tortuosity, will remain inexplicable, the water proceeding in a rectilinear course in the different windings of the bed in which it flowed, whilst the ice alone acted with the same energy upon every point of the basins which it filled.

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* I have no intention of describing the distribution of the erratic blocks which occur on the slopes of the Jura, inasmuch as they are generally known since the publication of the researches of Messrs Léop. de Buch, of Escher de la Linth, and of De Luc on the subject. I shall only remark, that their accumulation in different places does not at all agree with the theories which have been proposed to explain their transport. Thus the greatest accumulations, so far as I know, are found at an inconsiderable distance from each other, near the summit of Mont Auber, and at the bottom of Noiraigue, which are at very different levels, and are not in an ascending line whose summit would be Chasseron. On the contrary, upon the border of the several steeps of the Jura they are most markedly seen, and more particularly on the line which the depression of the upper beds of the *Portland* Rock produces along the whole length of the Neuchatel Jura, between the Castle of Neuveville, Fontaine-André, Pierre-à-Bot, Troirod, Châtillon, Fresens, Mutruz, &c.

† Any system of banking up or of breaking up, which can be imagined, will never explain so many facts common to so many valleys.

The objections which may be made to the theory of currents are all applicable, to a certain extent, to the theory advanced by several naturalists of their being conveyed by icebergs. It is no difficult matter to transport, in imagination, upon these floating islands great angular blocks even to the Jura; but the other particulars of this great phenomenon are not thereby better explained than by the help of the currents, were we even to admit with M. Elie de Beaumont that the water which fed them proceeded from the glaciers as the source.

Another objection of the greatest weight, made by M. Schimper to this theory is, the present state of the lakes of the great Swiss valley. If the blocks have been carried by the currents from the Alps to the Jura, these currents must naturally have passed over the lakes and the longitudinal and transverse valleys which occur between the two chains. How then has it happened that these lakes and valleys were not filled up, and how are we to explain the angular escarpments of their margins? However violent, rapid, and deep, these currents may be supposed, and were we even to imagine, contrary to every physical law, that they could have conveyed blocks of granite of the size of 50,000 cubic feet, as is that of Pierre-à-Bot, still they must have had their impetuosity diminished some time, and then the remains must have filled up some of the inequalities. Notwithstanding, there are no blocks to be found between the Alps and the Jura.

If, according to another hypothesis, the great boulders are conceived to have proceeded more gradually amongst great masses of mud and rubbish, so thick as actually to carry them along, how has it happened that these masses have not extinguished all the inequalities of Switzerland? How has it happened that the blocks alone should have been deposited after their arrival on the Jura, and the masses of mud which had been able to convey them so far, should then have flowed away, so as to leave them in their places?

There are still additional considerations which oppose themselves to the admission of any of these currents.

The erratic blocks of the Jura every where repose on polished surfaces, all those at least which have not been carried beyond the crests of our mountains, and which have not fallen to the bottom of our longitudinal valleys, as may be seen throughout the valley of the Creux du Vent. But they do not repose *immediately* upon these polished surfaces. Wherever the rounded pebbles which accompany the great blocks have not been removed by subsequent influences, it is remarked that small blocks, in other words pebbles of different sizes, form a bed of some inches, and sometimes even of many feet, *upon* which the great angular blocks repose. These pebbles are also much rounded, even polished, and are heaped up in such a way that the larger are above the smaller, and that these last often pass below into a fine sand, lying immediately over the polished surfaces. This order of superposition which is constant, is opposed to all idea of a transport by currents; for in this latter case the order of the superposition of the pebbles would have been precisely reversed. The presence of a fine sand at the surface of the polished surfaces, proves, beyond dispute, that no powerful cause could have been in operation, and that no important catastrophe could have affected the surface of the Jura, since the epoch of the transport of these alpine rocks, or, in other words, that the polished surfaces have not been dislocated or disturbed since the transport of the blocks. But as these surfaces form a large part of the northern bank of the lakes of Neuchâtel and Bienne, they prove, so far as they are concerned, that the Swiss lakes had previously existed; and the continuity of the moraines, upon the opposite banks of the Lake of Geneva, proves that this basin likewise was anterior to the transport, since it has preceded the formation of the moraines, as we shall very soon see.

In considering the intimate connection between the different facts which we have just been describing, it is manifest that every explication which does not *account at the same time* for the polish of the surface of the soil, for the superposition and the rounded form of the pebbles, for the sand reposing immediately upon the polished surfaces, and also for the angular form of the great superficial blocks, is an explication which is quite inadmissible as accounting for the erratic blocks of the Jura; and these objections forcibly apply to all the hypotheses respecting the transport of blocks with which I am acquainted.

I shall now proceed to that explanation of the phenomena which I consider the most plausible; and which is the result of my own views, together with those of M. Schimper, upon the subject. In glancing at many general questions which are connected with the explanation, I have no intention of expatiating upon them. I wish simply to demonstrate that the subject now before us has a relation to the most interesting and important geological inquiries.

The study of fossils has for some time led to very unexpected results, especially since it has assumed a physiological character; that is to say, since it has been recognised, that a progressive development exists in the whole range of those organized beings which have formerly peopled the earth; and since epochs of renewal have been recognised throughout the whole. Those individuals who have admitted this progression ought not now to entertain any fears in prosecuting these consequences to their legitimate limits; and the idea of a uniform and constant diminution of the earth's temperature, such as is now sometimes admitted, is so contrary to every physiological idea, that it must be strenuously repelled, to make way for another, viz. that there has been a diminution of temperature, which has been accidental in relation to the development of the organized beings that have appeared and disappeared one after the other at determinate epochs, maintaining itself at a particular mean temperature during a given era, and diminishing at certain fixed epochs.

As the development of individual life is always accompanied with that of heat, since its continuance establishes a certain equilibrium of longer or shorter duration, and since its extinction produces an icy coldness, I conceive I deduce only legitimate inferences, when I conclude that the same phenomena occurred upon the globe: that the earth, when it was formed, acquired a certain very elevated temperature, which progressively diminished during the different geological formations; that during the continuance of each of them, the temperature has not been more variable than that of our globe since it has been occupied by its present inhabitants, but that it has been at the epochs of the disappearance of these inhabitants that a fall in the temperature has taken place, and that this fall has been beneath the temperature which prevailed in the subsequent epoch, and which re-appeared with the development of the newly animated creatures which were called into existence.

If this theory be correct, and the facility with which it explains so many phenomena which have hitherto been deemed inexplicable, induces me to believe that it is; then it must follow that there has been, at the epoch which preceded the elevation of the Alps and the appearance of the existing animated world, a fall of temperature far below that which prevails in our days. It is to this fall of temperature that we must attribute the formation of those immense masses of ice, which must universally have covered the surface, where we find these erratic blocks along with rocks which are polished as are ours. It is also, unquestionably, this extreme cold which has enveloped the Siberian mammoths in ice, has congealed all our lakes, and accumulated the ice as high as the ridges of our Jura, which existed before the elevation of the Alps.

This accumulation of ice above all the hydrographic basins of Switzerland may easily be supposed, on reflecting that when lakes are once frozen to the level of their emerging current, the running waters no longer drain off, and those of the atmosphere, augmented by the vapours of the southern regions, which, under the circumstances, abundantly precipitate themselves towards the north, must have most rapidly augmented the extent, and raised the level even to the height which has already been established by the foregoing facts. The winter of Siberia was for a time established upon a soil previously covered with luxuriant vegetation, and peopled with great Mammalia, whose fellows in our day inhabit the warm regions of India and Africa. Death enveloped nature in its winding-sheet, and the cold reaching its extremest limit, gave to this mass of ice, at the maximum of tension, the greatest hardness it could acquire. When any one has frequently witnessed the congelation of a lake, he can then form a conception of the vast resistance of ice in this condition, and to what immense distances hard bodies which are thrown upon its surface may glide in consequence of even a feeble impulse.

The appearance of the Alps, the result of the greatest convulsion which has modified the surface of our globe, found its surface covered with ice, at least from the North Pole to the shores of the Mediterranean and Caspian Seas. This upheaving, by raising, breaking, and cleaving in a thousand ways, the rocks which compose the prodigious mass that now forms the Alps, at the same time necessarily raised the ice which covered them; and the debris detached from so many deep upbreakings and ruptures, naturally spreading themselves over the inclined surface of the mass of ice which had been supported by them, slid along the declivity to the spots where they were arrested, without being worn or rounded, since they experienced no friction against each other, and even when arrested came in contact with a surface so smooth; or after being stopped, they were conveyed to the margin, or to the clefts of this immense sheet of ice, by that action and those movements which characterise congealed water when it is subjected to changes of temperature, in the same manner as the blocks of rock which fall upon glaciers, approach their edges in consequence of the continual movements which the ice experiences, in alternately melting and congealing at the different hours of the day and seasons of the year. These effects ought to be described in detail; but as they are partly known I shall not dwell upon them.* I shall only remark, that the power of the action, so far as the ice is concerned, is immense; for these masses, continually moving upon each other, and on the surface, bruise and grind down every thing moveable, and polish the solid surfaces on which they repose; at the same time that they push before them all that they encounter, with a force which is irresistible. It is to these movements we must attribute the strange superposition of rolled pebbles, and of sand which immediately repose upon the polished surfaces; and it is unquestionably to the grating of this sand upon these surfaces that the fine lines which we find are owing, and which would never have existed if the sands had been acted upon by a current of water: for neither our torrents, nor the stormy waters of our lakes, produce any thing like this upon the very same rocks. As to the longitudinal direction of these fine lines, and of the furrows which are observed upon the polished surfaces, it ought to be observed that they must have resulted from the much greater facility which the ice had in dilating itself in the direction of the great Swiss valley, than transversely, confined as it was between the Jura and the Alps; the phenomenon itself commencing only with the retreat of the ice, at the time that the Alps appeared. I have not the slightest doubt that the greater number of the phenomena which have been attributed to vast diluvial currents, and in particular those which M. Seefstrom has recently made known, have been produced by ice.

Upon the elevation of the Alps, the surface of the earth would be reheated, and the caloric disengaged on every side would produce the melting of the ice, which would gradually retire into its present domain. Clefts would first be formed in those places where the ice was thinnest, that is to say, on the summits of the mountains and the hills which were covered by it, afterwards upon the most saliant parts of the plain; valleys of drainage would then be excavated at the bottom of these clefts, in localities where no current of water could flow without being inclosed within congealed walls; and when the ice had completely disappeared, the great angular blocks which had covered its surface, or had fallen into the clefts, would be found upon a bed of small rounded pebbles, under which is usually found a layer of sand. In melting from the surface, the ice must necessarily have continued longest in the depressions of the country, in the little longitudinal valleys which are formed by the different zones of the strata of the Jura, and at the bottom of the lakes; and it is undoubtedly to this circumstance we are to attribute the extraordinary position occupied by so many of these blocks, which are perched up, scarcely in equilibrium, upon the highest points of rocks; and also their constant absence in the hollows, where they are not found, except at least where fresh momentary expansions of the ice were able to precipitate them.

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* M. Schimper has written a most interesting work upon the effects of ice, to which I should have been most happy to refer if it had been published.

So long as the level of the ice on the Jura had not fallen below the line of Pierre-à-Bot, the blocks which were yet spread over its whole surface, might continue their descent towards the Jura; but so soon as the ice became thin over the plain of Switzerland, it must have very speedily disappeared, and have only left portions in the deep valleys, and in the basins of the lakes, that is to say, it must have been soon confined to the lower valleys of the Alps.

In reflecting upon what must necessarily have occurred upon this disappearance of the ice, we are naturally led to think that the transport of the rolled pebbles of the valley of the Rhine, and the deposition of *Löss*, must have been among its first effects; and this is confirmed by the facts, that these pebbles are the same with those which we found along with our blocks, and that the *Löss* is evidently the result of the detritus of the *molasse*. The frequent *débâcles* of the ice could only at that time convey blocks upon the masses of ice to great distances, or carry them farther in their current.

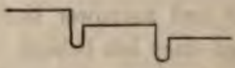
The melting and maceration of the ice and its repeated congelation in cold weather have produced many other geological effects, which it is difficult to account for as produced by any other causes. Without again recurring to the valleys of drainage or erosion, I may mention those deep furrows which are not fissures, and which have above them plains of great extent; also those small lakes which are sometimes formed near the edges of the glaciers, and which so affect the small stones that are accumulated at their margins, as to impress upon them the appearance of stratification; or again, the analogous phenomena which are observed upon the limits of different stations where the immense sheets of ice have successively stopped in their retreat; and likewise the dispersion of the bones of the Mammalia at the diluvian epoch, without their being at all rolled or broken, and in short, a number of other particulars which have no interest except when we embrace the whole of the question.

From this moment the surface of the earth must have been afresh subjected to the influence of the regular succession of the seasons. Then appeared the first spring time of the animals and plants which flourish in our days. The ice had retired to the foot of the Alps, and from their summits it began to receive fresh reinforcements. Speedily it reached its last retreats, where it is ever oscillating, sometimes gaining in extent, and forcing the blocks before it, and sometimes again retiring within narrower and narrower limits. At each step of ground they abandoned they left behind them, as the existing retreating glaciers now do, some of those long dykes of blocks which still exist in the Alpine valleys. Soon, too, the lakes themselves would melt, the waters would assume their present courses, the valleys of the Alps would be drained, and there remained no more ice, the product of former congelation, except on the summits of snow-clad mountains.

It would be a great mistake, therefore, to confound the glaciers which descend from the summits of the Alps with the phenomena of the epoch of that extensive ice which had preceded their existence.

The phenomena of the dispersion of erratic blocks, then, ought not any longer to be regarded in any other light than as one of the circumstances which have accompanied the vast changes occasioned by the fall of the temperatures of our globe previous to the commencement of our epoch.

The admission of an epoch of cold, which was so intense as to cover the earth to such distances from the poles with so vast a mass of ice as we have been contemplating, is a supposition which appears in direct contradiction with those well known facts, which shew a considerable cooling of the earth since the most remote period. Nothing, however, has proved that this refrigeration has been constant, and that it has occurred without oscillations. On the contrary, whoever has been in the habit of studying nature in a physiological point of view, will be much more disposed to admit that the temperature of the earth has been maintained, without any considerable oscillations, to a certain degree, during the whole period of any geological epoch, as is occurring in our own epoch, since it has diminished suddenly and considerably at the termination of each epoch, a change accompanied by the disappearance of the organized beings which characterized it, that it may rise again with the appearance of a new creation at the commencement of the following epoch, although at a lower degree of mean temperature than the preceding one, so that the diminution of the temperature of the globe may be expressed by the following line :—



Thus, the epoch of extreme cold which preceded the present creation, has only been a passing oscillation of the temperature of the globe, somewhat more considerable than the periodic refrigeration to which the valleys of our Alps are subject. It was attended by the disappearance of the animals of the diluvian epoch of geologists, as the mammoths of Siberia still attest, and preceded the uprising of the Alps, and the appearance of the animated nature of our day, as is proved by the moraines, and the presence of fish in our lakes. There was thus a complete separation between the existing creation and those which have preceded it; and, if the living species sometimes resemble in our apprehension those which are hid in the bowels of the earth, it nevertheless cannot be affirmed that they have regularly descended from them in the way of primogeniture, or, what is the same thing, that they are identical species.

By prosecuting these views, we may anticipate the time will come when we shall be able to determine the geological period at which the sun began to exercise an influence upon the surface of the globe, so considerable as to produce the differences which now exist between the different zones, without these effects being neutralized by the influence of the internal heat, from which the earth must for a time have enjoyed a very uniform temperature over all its surface.

This theory, I fear, will not be adopted by a number of our geologists, who have settled and confirmed opinions on the point; but I anticipate it will be with this question, as with many others which assail old and established views. At all events, whatever opposition it may experience, it will remain true that the numerous new facts relating to the transport of blocks which I have pointed out, and which may be studied so easily in the valley of the Rhone and the environs of Neuchatel, have brought the discussion into wholly different ground from that on which it has hitherto been debated.

When M. de Buch for the first time affirmed, in opposition to the formidable school of Werner, that granite is of Plutonic origin, and that the mountains had been uplifted, What did the Neptunists say? At first he sustained his position alone; and it has only been by his defending it with the innate powers of genius, that he has made it triumph. It is happy for us, that in scientific discussions, numerical majorities at first have never decided any question.

The form into which I have thrown these observations will, I trust, banish all discussion on the subject at the present moment; and as, at the same time, I cannot hope that I have convinced every one of the truth of my views who have heard them for the first time, I propose the Geological Section as the most suitable for any discussion which may follow. I shall then make it my business to meet any objections which may be started; and, for the sake of truth, I earnestly solicit them.

Notice of Observations made on the Rapidity of Motion, and on the Duration of the Stoppages on the Liverpool and Manchester Railway.[*] By Edward Sang, Esq. F.R.S.E., late V.P.S.A., Lecturer on Natural Philosophy, &c. Edin.

(Communicated by the Society of Arts.)

If any one had asserted a century ago, that vehicles carrying a dozen of passengers would be drawn at the rate of ten miles an hour from London to Edinburgh, not merely now and then, but every day throughout the year, he would have been ridiculed as talking of an impossibility; so ten years ago, the prospects of ever being able to travel at twenty or thirty miles an hour, seemed almost as unreasonable.

The velocity of twenty-five miles an hour is, however, already attained; and speculations are afloat as to whether even that prodigious rapidity may not yet be exceeded. That a far greater velocity than twenty-five miles an hour can be obtained by locomotive engines on a well laid rail, is certain; for at present that velocity is steadily maintained with a train of ten or thirteen waggons, each having from twenty to twenty-four passengers.

The question is, not whether such a velocity as forty or fifty miles an hour can be had, but whether it be practicable at such a speed to regulate and command the machinery; and whether the increased expense may not counterbalance the advantages of such a rate.

To the first of these questions I mean at present to attend.

During the last summer, I had occasion to travel twice every week from Liverpool to Manchester, and back; the total extent of my journeys on the rail being somewhere about 800 miles.

At the first, the rapidity with which the objects in the vicinity of the road were passed, and the apparent rotatory motion which those even at a distance seemed to have, caused an unpleasant and confused sensation, which was removed at once by looking at the parts of the train, or at objects several miles off. One or two trips, however, served to accustom the eye so well as to allow of an inspection of the objects at the very edge of the rail-road, and then the sensation of rapidity, which hardly ever leaves us while travelling in a coach, disappeared entirely; and it was only by looking at the passing objects, or by attending to the rattle of the steam, that I could form an idea of the rate.

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[*] Read before the Society for the Encouragement of the Useful Arts in Scotland, 15th February, 1837.

With the velocity of twenty-five miles an hour, even when exposed to the current of air, there was not the slightest approach to any feeling that would lead me to suppose that four or five times the velocity would disable the engineers from directing and managing the train. Such was the result of my own observation, and it was fully borne out by the experience of the men. I may cite two instances of common occurrence.

When the train arrives at the foot of one of the inclines, the banking engine follows to assist it up. Now one would be apt to imagine that, for the purpose of attaching the new engine, the train would stop, or that if it did not, there would be a concussion when the banking engine comes in contact.

So completely, however, are these powerful engines under the control of their directors, and so well are they managed, that a passenger in the train who is not aware of what is going on from ocular perception, is altogether unconscious of any change. I frequently watched this operation, but on no occasion could perceive the slightest shock, even when situated only one or two carriages from the end of the train. The only thing noticeable was, that the springs which keep the carriages apart were compressed, and that their shadows on the ground approached each other.

On one occasion, the banking engine had got before us on the incline; as the hooking of it on in such a situation was a much severer test of the skill with which matters are managed, I attended closely to the operation. We were going fully twenty-five miles an hour. The banking engine gradually slackened its rate, and allowed the train to gain upon it, until it could be hooked on,—that done, more steam was given, and we proceeded with its assistance, yet not the slightest shock was felt in the train.

These facts are sufficient to shew, that much greater rapidity is practicable, so far as the power of managing the apparatus is concerned.

As to the rapidity itself I made some observations. Twenty-five miles an hour is not so very rapid. Over and again I saw bees not merely keep pace with us, but fly round and across the coach, and that not by help of any current of air which might be supposed generated, but at several feet distant from the train. At times, too, specimens of the *Libellula grandis* kept up with us over a stretch of half-a-mile; while the smaller birds, such as the linnet, were unable to cope with the steam. Once I almost caught one, which, flying with all its might, remained opposite the window for a few seconds.

The effect of long-continued travelling on myself, was to give me a perfect distaste for horse-coaches. It seemed, on getting out of the steam-carriage and into the hackney-coach, that we were standing still, or moving at a snail's pace; and after four or five trips, the thing became so intolerable, that I preferred to send my luggage by an omnibus, and to walk into town.

If a rail-road be regarded only as a means of communication between two distant towns, I should have no hesitation in saying, that a rate of even one hundred miles per hour could be maintained with perfect safety to the passengers; but it is different if passengers have to be let out at stations along the line, for then the trouble and expense of stopping the train comes to be considered.

To form a correct notion on this head, I noted, during two journeys, the times of arrival at each quarter mile mark, and also the instants of starting,—of letting off the steam,—of stopping. From these observations, I find that from the instant the steam was let off, till the train fairly stopped, there elapsed from 60 to 152 seconds, the average being about 90 seconds, thus occasioning a loss of time of 45 seconds each stop; but again, the train did not acquire its full speed after the steam was let on, for some 150 seconds,—indeed its rate kept sensibly increasing even at the distance of a mile and a half, or two miles, owing to the increased violence of the fire; here, then, is another loss of 75 seconds, making two minutes of delay for each stop, independently of the time during which the train is actually at rest. We may say, at an average, three minutes in all.

If the stops be very frequent, the engine is unable to bring the train to full speed during the interval, and the delay becomes proportionally vexatious; and, of course, if the rapidity of motion be augmented, the loss of time in stopping is so too. For this reason, it appears to me that very great velocities are incompatible with supplying the means of conveyance to places intermediate between the coke and water stations, unless we can contrive some means of letting off the passengers without stopping the whole train. This could be accomplished by placing all those destined for a particular place in one carriage, and allowing that to fall off from the train half a mile or quarter of a mile before arrival; but then there would be the annoyance of gathering up these stray waggons, while no means would be afforded of taking up passengers.

This subject will undoubtedly become important when greater extent of rails is laid down, and greater rapidity aspired at, but in the present state of matters, the expedient of having a first class train to go straight through, and a second class train to stop at intervals, seems to answer every required end.

Theory of Granite, and the other Massive Rocks; together with that of Crystalline Slate; proposed in Lectures in Geology, in the University of Christiania in Norway, in the year 1836. By B. M. KEILHAU, Professor of Mineralogy.*

(First Extract.)

HAVING described in a general way the stratified rocks of the younger transition series,† we must now, according to our usual method, inquire if massive ("abnormal") rocks also occur within the limits of that formation, and proceed to treat of them in the same general manner. We shall first describe the constitution of this group in Norway, as it is precisely here that these formations are presented to us in the most instructive manner, and are so exhibited as to afford illustrations by which we may more easily rise to general considerations. The clearness of the geological relations of the above-mentioned rocks in our country is so great, that their study will, I hope, lead us to fixed views regarding the mode of formation of granite itself, and generally of the other unstratified rocks, with regard to which we have hitherto for the most part been guided by the assertions of the prevailing schools of former times, as we did not venture to form for ourselves an opinion on the subject. It will be necessary for us to enter into a great many details; but the object is of the highest importance for science; and besides this, I venture to hope that the greater part of the special facts which are to be adduced will seem the less tedious, because they relate to phenomena occurring in places well known and easily accessible in our own neighbourhood, and which therefore every person, in order to remove all doubt, may examine for himself. I shall at the same time restrict myself as much as possible to generalities; and indeed a great many extremely interesting data cannot at present be taken into consideration, with respect to which, those who wish for more accurate information, must have recourse to descriptions already published, or about to be so in a short time.

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* This interesting memoir is translated from an article in Danish, in the first Number of the "*Nyt Magazin for Naturvidenskaberne,*" recently commenced at Christiania. We are indebted for a separate copy of the essay to the kindness of its distinguished author.

† I divide the transition class into two groups, a younger, rich in limestone and petrifactions; and an older, comprehending especially sandstones, and which never, or at least very rarely, contains fossils.

There are two tracts of country which we have to take into consideration, 1st, that which, for the sake of brevity, I have termed the *Transition Territory of Christiania*, the boundaries of which are shewn in the map about to appear in the first part of a work called the *Goea Norvegica* to be published by me, with the assistance of the Royal Norwegian Society of Sciences, and also in a map which forms plate 7th in my Darstellung der Uebergangs Formation in Norwegen, Leipzig, 1826; 2d, that which commences somewhat further to the north, and extends on both sides of the Mjösen, so that we may conveniently denominate it the *Upland* (oplandske) *Fossiliferous Transition Territory*. As in former discussions of this kind we have been in the habit of adopting the old usual method of proceeding, we shall first endeavour to describe the phenomena. Our commentary shall follow afterwards, together with the changes or additions which may be necessary.

Among the members of the fossiliferous group of the transition series occurring in these two districts are—clayslate, limestone, greywacke, greywacke-slate, transition-sandstone, and sandstone-slate. Limestone and clayslate alternate with each other, or the former occurs in more or less extensive beds in the latter; sandstones and sandstone-slates lie regularly uppermost in the whole series; greywacke and greywacke-slate are, in fact, peculiar to the northern portion of the Mjösen, and they there form broad zones, running from east to west, which alternate with zones of limestone. The whole repose in an overlying and unconformable position upon the outgoings of the primary deposits, viz. on the ends of the strata, consisting of micaceous gneiss, hornblendic-gneiss, mica-slate, and hornblende-slate, which have a high inclination, and end, or are as it were cut off, within a surface, which, with greater or smaller hollows and heights, extends itself over many hundred square Norwegian* miles. In very few places transition strata lie horizontally over their primary basis, or, indeed, in conformity with its surface. This is, for the most part, only to be observed near the boundaries of the territory (geologically speaking), where, when the primary basis has a somewhat level surface, the superimposed strata end in an abrupt termination, which proves that they have extended considerably farther over the primary basis, but at a later period have been destroyed, or partially carried away —changes which also become apparent in this way, that we often still see, for a long distance on the front of precipices, a sort of thin coating of clayslate on the outgoings of the gneiss, or observe other traces on the surface of the primary basis, of a formerly existing covering, continued from these abrupt terminations. But in many places abnormal formations have intruded themselves between the basis and the superincumbent strata (we begin now to speak in the style of vulcanists), and dislocations of these strata, deviations from the original more or less horizontal position, present themselves as their most general condition. We may safely state, that the dip is generally greater than 45° in these districts. In many places the strata rise to perpendicularity; and, in my opinion, it cannot be denied that this position occurs in the Christiania territory, within whose boundaries we find so many, and such remarkably large examples of these abnormal masses, which we shall now investigate more closely.

The unstratified mountain rocks which come within the circuit of our formation are granite and syenite (either with or without zircons); a number of greenstone rocks, viz. diorite, granitoidal amphibolite, and aphanite; also red and black porphyries of many varieties; basaltic rocks and amygdaloid, together with porphyry-conglomerate. In the Upland territory, which, in comparison with that of Christiania, is extremely deficient in these kinds of mountain-rocks, porphyry chiefly occurs, and also somewhat smaller portions of granite and greenstone.

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* A Norwegian mile is equal to about seven English miles.—Edit.

Granite and Syenite.—Granite and syenite exhibit precisely similar relations, and frequently pass into each other. They are to be regarded as but one formation. In the Christiania territory they appear at the surface in tracts, which, taken together, amount to more than a third part of the whole area of that portion of country, and two of the districts which they form have each a superficies of upwards of 200 square English miles; with respect to which it is to be remarked, that the form of these districts is generally not particularly stretched out longitudinally, so that we could not, for instance, compare them with the Cleveland dyke in England, which also occupies an extraordinary extent, but only in one direction. Neither could we think of comparing these large expanses of granitic masses with those great horizontal sheets of trap, which, we are told, in Iceland, for example, present a still larger surface. As to the manner in which these masses join with the stratified rocks, it is in fact most in accordance with the mode of occurrence of vertical veins: at many points the direct contact is so exposed, of the granite or syenite on the one side, and the transition strata on the other, that we can convince ourselves that the massive formation, *on the whole*, neither lies over nor under the adjoining strata, but stands at their side and cuts them off. The continuity of the masses downwards can moreover be proved in the clearest manner by this, that in many places at the boundary of the transition-formation, where, as we have said, the Neptunian strata are no more to be found, we can observe granite or syenite in conflict with the primary basis, inasmuch as the latter is penetrated by the former, so that it is clear that these granitic masses rise up from the very foundation of the transition strata. Both in the primary series and in the superincumbent formation, there are numerous branches shooting from the main masses into the contiguous rocks, and some of these side offsets, issuing out in this manner, are so large as in some places to exhibit the appearance of sometimes an overlying and sometimes an underlying position of the massive formation in respect to the transition strata; but nevertheless the relative situation upon the whole, as we have already remarked, is to be regarded as juxtaposition, a condition which, taken in connection with the ramifications springing from the chief masses, naturally leads to the supposition of a previous liquid condition of these rocks, in which state they were pressed upwards from the interior. Yet, with this view of the subject, one cannot possibly avoid being struck with a high degree of astonishment, in considering the magnitude of these masses. In the whole of the wide space that they occupy, there is generally no trace of divisions or of such separate portions as could indicate that several ejections had taken place. Every one of these enormous masses must be considered as the result of one single eruption, or, so to speak, as one gush from the furnace, one casting in a foundry. In taking this view, it must also appear surprising that these granitic masses, which must thus be considered as thrown up through canals of several miles in breadth, do not nevertheless form particularly high mountains. Some of their highest summits may be 2000 or 3000 feet above the level of the sea; but the unevenness arising from these is very inconsiderable; for if one were placed so that with one glance he could look over the whole surface of these masses,—such, for instance, as have an extent of 200 square English miles,—it would appear to him that this surface was tolerably level, that is to say uneven only on the small scale. Such a form cannot be explained but on the supposition, that this open gap was filled just to the edges, that by a singular chance there was no more of the molten mass forthcoming, and that thus there was not enough to furnish a rising on the top.

But these circumstances will have but little weight with him who is inclined to consider the masses in question as the products of the action of fire; for, in addition to them, he will find abundance of phenomena which are generally received as proofs of the Plutonian origin of such rocks. He will find many highly inclined, nay perpendicular strata, at most of the places on the granite boundaries; he will observe dark-coloured compact limestone, even with its petrifactions, converted into light-coloured crystalline marble; he will at the same points see clayslate altered to flinty slate or striped jasper, and he will observe that these changes proceed farthest where there is an immediate contact with granite or syenite, but that they become less and less considerable the more remote the point from these rocks; finally, he will also meet with a multitude of contact minerals at the same boundaries, such as copper and iron ores, calcareous spar, garnet, &c. We have said that these phenomena are to be found in great abundance, but the expression ought rather to have been, that they here exhibit a more striking development than at any other known locality; the altered strata have been here acted on by contact with the granite, to the hitherto unexampled distance of one-sixth of a Norwegian, or one-fifth of a geographical mile, that is to more than one English mile. As to what relates to the contact products, it will be sufficient to mention, that of about sixty iron mines, setting the smaller experimental workings out of consideration, which occur in the granite and stratified districts of the Christiania territory, there are twenty of them to be found immediately at the boundaries of these two districts, and about half as many in the tracts near the boundaries, where the limestone and slate have undergone changes by contact with granite.

Porphyry without Quartz ; Amygdaloid and Basaltic formations.—We proceed now to speak of another group of these massive rocks. As granite and syenite are, geognostically speaking, mere unessential modifications of one principal type, so in this case, when taking only a general view, the black porphyries, or, as we may better express it, the porphyries without quartz (*quartslöse porphyrer*), the amygdaloids, and the basaltic formations, are to be regarded as one group. They compose along with that porphyry-conglomerate, which is also to be ranked among the massive formations, two or three reciprocally similar districts, all of which in every place present the same relations to the other geognostical districts and mountain-rocks of the territory. These porphyry districts, as we may call them for the sake of brevity, are not much less extensive than the granitic as to the area they occupy; but their masses are much more superficial than those of the granite, and so have undoubtedly not nearly such enormous cubic bulk. We have opportunities of observing in many places that they form beds of very great size, *upon* the stratified transition-rocks, so that they do not descend into these, at least with their whole horizontal extent. In several other respects also, they present different features from the granitic masses. They often exhibit in the form of beds pretty horizontal offsets; we find in most places, that in the strata lying immediately under them there are no dislocations; no substantial alterations are observable in the transition-rocks at their junction with the porphyry, and contact minerals at the boundaries are awanting; lastly, it is a remarkable relation of the porphyry, and one which seems to be the result of a very fixed rule, that it reposes only on sandstone, and thus, in general, can only occur where this member of the transition group is present: in the Upland territory, which contains this sandstone only at one locality, it is precisely at that place, and there alone, that porphyry is found. But be that as it may, so far as it concerns the part we have for the present assumed in the discussion, we need not be embarrassed; we have an all-important fact in reserve, to overturn every doubt which can be started as to the conclusions, that these porphyries have risen up from the interior and have been poured over the beds of sandstone,—that it was so only over the sandstone, may be an accident. This fact is analogous to those by which we have strengthened our volcanic faith, viz., the phenomena exhibited in the mutual relations of the trap-rocks and the brown coal-deposit of Iceland.* In the present case, we can ourselves easily be witnesses to the remarkable phenomenon of a trap mass spreading itself out over Neptunian strata in the form of a more or less horizontal bed, and at the same time occurring as a more or less *vertical vein* in the same strata; we can observe with tolerable distinctness, the union of the vein with the bed-like mass, which, indeed, no one, it would be supposed, can deny, was raised up in a liquid state through the vein-fissure (gangklöft) proceeding from beneath! It is in the neighbourhood of Holmestrand that I have observed this appearance, and we have here a true example of the so-called "*Stilk*" (literally stalk) a phenomenon so anxiously sought for under basaltic summits, &c. This strong presumptive proof, however, has some material defects, which we, in our triumph, must not overlook; at the sides of the vein are beds of sandstone, which are in all respects unchanged, and *friction conglomerates ("Frictions Conglomerater")* which ought certainly to be expected here, are not to be found. As to what relates to the conglomerates, which, as has been mentioned, occur among the rocky masses composing the porphyry districts, they never contain any other fragments but those of porphyry, and they partly lie in such a manner involved in the midst of true prophyries, that they cannot possibly be regarded as friction-conglomerates.

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* The geological constitution of Iceland appears to me extremely important for the theory of Trap-rocks. The phenomena mentioned in the text are described by Krug Von Nidda.

Eurite-Porphyry, Greenstones, &c.—The remaining abnormal formations of the territory of Christiania, occur in far less considerable masses than those of which we have hitherto treated. They have sometimes the form of beds; and sometimes that of veins, according to the usual rule, that allows us to regard each of them, taken as a whole, as a vein. The red porphyry, or, as it is termed in petrographical descriptions, eurite-porphyry, is, for the most part, intermingled with the stratified rocks in such a manner, that the flat masses which it forms are parallel with the strata; but there are also to be found some individual examples in which it presents the most distinct veins. It is remarkable, that, in by far the greatest number of cases, it is confined to the places where the clayslate formation occurs in the form of alum-slate, and it is also a striking phenomenon, that the alum-slate never seems altered when in contact with the porphyry, even where it occurs having a thickness of not more than a line, as may be distinctly seen in several places in the immediate neighbourhood of Christiania. Sometimes there is no slate present between the beds of porphyry, which nevertheless occur as distinct beds, and often succeed one another in pretty considerable number. In such cases it is perfectly evident, that they can by no means be the actual results of lateral injection; according to the Plutonian view of the subject, we can only suppose a repetition of effusions corresponding to the number of the beds, by which the matter of the porphyry each time flowed out over the uppermost of the already existing, and at that time perfectly horizontal beds. Although many of these bed-like masses of porphyry, with regard to their boundary relations and their position, answer in the most complete manner to the conditions which are so far required for true beds; yet, there are not wanting deviations in this respect. On the western boundary of our geological territory, there is a bed of porphyry seven English miles wide, at whose regular intercalation in the alum-slate for great distances we cannot but be surprised; but at one place there is distinctly to be observed, a short and thick outrunner, by which the mass descends into the subjacent bounding rock; irregularities of other kinds are to be found in others of these porphyry masses.

While eurite-porphyry most commonly occurs in the form of more or less perfect beds, and is found only as an exception in the form of veins, the reverse is exactly the case with regard to the greenstones, viz. Diorites and Aphanites. These rocks, which in some places represent basaltic masses, are to be seen at many points near Christiania, forming nearly vertical veins, which pass almost at right angles through beds of limestone or clayslate. These continue likewise beyond the boundaries of the transition series, and a few of them can actually be traced into the primary formations. It must be with the greatest possible satisfaction that the supporters of the pyrogenetic theory of the massive mountain-rocks survey these veins, for not only does the last-mentioned fact support their opinion, that the greenstones have been pressed up through clefts in the primary formations to and through the transition strata; but, further, portions of gneiss and other fragments torn from the primary rocks, are found in the greenstone veins, and that too even within the limits of the transition territory. In some places, also, we find that the slate is indurated, and the limestone impregnated with silica, at the junction with these masses of greenstone; and we are acquainted with one vein of this sort which is perfectly analogous to that near Aschaffenburg in Germany, which we cited when treating of the variegated sandstone formation, and which is accompanied by a separating mass (*bestegformig Masse**) of brown ironstone. Like it, our vein has immediately at its side a continuous mass of ironstone (magnetic iron-ore) considerable enough to be regarded as an object for mining operations. The mine which owes its existence to this remarkable appearance, and where one can observe the phenomenon in a convenient and convincing manner, is that of Aaserud, below the Eidsfoss works. We have then in these greenstone veins three of the appearances which are considered as proofs of their Plutonian origin; the included fragments, the alterations of the bounding strata, and the formation of particular substances at the line of contact. A fourth phenomenon, which is regarded as the consequence of the supposed eruptions, viz. disturbances in the position of the neighbouring strata, has hitherto not been any where observed.

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* *Besteg* in German and Danish, and corresponding to *lisière* in French, is the term employed to denominate the bed or layer which sometimes separates a vein from the rock in which it is contained.—Edit.

It has been stated that greenstone also assumes the form of beds. This, it is true, cannot be denied; but from some examples it would appear that, if we could trace such masses far enough, we should probably in most cases find deviations, in consequence of which they would be better described by the term *filons-couches*, employed by some French geologists. Thus, there is an example on Langöe, near Holmestrand, of a mass of greenstone, which for a long distance lies quite parallel to the slightly inclined strata of the hanging and lying sides; but afterwards it suddenly deviates downwards on the lying side, and then this vein-like included mass is continued again as a bed within the stratum which formerly belonged to the lying side. There are cases, besides, where masses which for a long way have continued as beds, at once forsake this form, and become forked, which, however, often happens in such a manner that the portions separated by the bifurcation again run on as thinner beds. According to the eruption theory, these must be admitted to be the results of pressings in from the side. It deserves here to be remarked, that greenstone never exhibits those generally thin ramifications penetrating the contiguous rocks in all directions, which are so common with granite and syenite. The same is the case also with the porphyries. As the cause of this, it would be easy to assign a difference in the degree of fluidity in the respective formations at the time of their appearance, viz., that the greenstone and porphyry were more viscid, that is, less fluid than the granite.

Rhombic Porphyry.—We have still one species of abnormal masses to bring forward of those which occur in the limestone and clayslate, and also in the sandstone of the transition territory of Christiania. These masses present porphyries which on the one side approach the type of this rock, which is dark coloured, and contains no quartz, and which forms the principal mountain-rock of the great porphyry districts; and on the other side approach the red eurite-porphyry, which occurs only in a subordinate manner in the stratified districts. Among the varieties of the first kind, Von Buch found the prototype of his rhombic porphyry; and a variety of Brongniart's brownish-red porphyry (*porphyre brun-rouge*) or melaphyre, which that author says is to be found on Tyveholm, close to Christiania (*Brongniart's Tableau*, p. 350, and *Classification*, p. 107), belongs also to this division. The masses, consisting of rhombic porphyry, exhibit in Christiania Dal, a third sort of form peculiar to themselves, when we regard *eurite-porphyry*, with its mode of occurrence *in beds*, as the first kind, and *greenstone*, with its mode of occurrence *in veins*, as the second. In the vicinity of the town we see the rhombic porphyry partly in perfectly irregularly bounded masses, lying in a series of strata composed of clayslate and limestone. These masses are for the most part extended in the same direction as the greenstone veins; but they are comparatively short, and never exhibit even and reciprocally parallel *salbandes** like the greenstones; on the contrary, we observe the rhombic porphyry branching out into the bounding rock in thick wedges, or broad round offsets. In other localities this porphyry presents itself in pretty regular veins, but it is there that, in its petrographical characters, it approaches the eurite-porphyry. An example of rhombic porphyry as *a vein in the primary rocks themselves*, where the latter are no longer covered by the transition series, is to be seen at two places; and at least in one of these it is manifest that the vein extends without interruption for a long distance into the transition territory, where it appears in strata of limestone and clayslate. The same vein,—and one of the places where this is to be seen is on the promontory of Noesodden,—exhibits another extremely remarkable relation, which is best to be observed at the locality we have just named, and where it occurs in the primary mica-slate; between the mass of porphyry and the mica-slate, there runs on each side of the vein a zone or separating layer (*Besteg*) of greenstone, about a foot in thickness; and, indeed, at many points of the same vein lying far from the one specified, the greenstone shews itself in the same way and having the same thickness. This singular phenomenon, which, however, is not peculiar to the mass of rhombic porphyry here mentioned, but occurs in the case of several other abnormal formations of the territory, must, according to the eruption theory, be explained in the following manner: Either the greenstone must be considered as belonging to a later intrusion, during which it found its way up through clefts, which were formed by the separation of the porphyry from the adjoining rock on both sides, or it must be regarded as the effect of an earlier eruption, and was subsequently cleft through the middle, when the porphyry pressed upwards and filled the newly opened fissure.

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* The two surfaces which form the walls of a vein are termed *saalbandes* or *salbandes*.—EDIT.

We have now given the promised description. Nothing, so far as I am aware, which can be adduced in favour of the volcanic theory of the massive mountain-rocks, has been passed over;* nor is there, on the whole, any thing wanting which is generally brought forward in statements of this kind; unless it be, that we have not spoken of slags or of alterations by glazing and vitrefication; but phenomena which are no where to be observed, could not of course be introduced among actual facts.

It is to be further remarked as to the representation we have given, that the view which it exhibits to our eye, although it is undoubtedly incomplete, as we shall soon see, yet, nevertheless, is in no small degree of exactness, a resemblance of the original. At least a statement framed under the influence of the other principal theory, viz., Neptunism, which considers granite as a crystallization formed in and by means of water, would not, by any means, have come so near the truth, and could scarcely have included so many actual facts in its favour, as have been here brought together. As to what relates to the incompleteness of our volcanic description, this is partly owing to the circumstance that many of the most important phenomena are altogether passed over; and partly to this, that more of those which have been touched on, have had representations given of them when they were but half examined, and which often make them appear very different from what they really are; but we could not do otherwise than make ourselves accountable for these defects, unless we were to drop our proper part. It happens, unluckily, that a preconceived theory renders an observer blind to the facts which do not harmonize with his own conclusions; I will not make any remarks prejudicial to those observers who may not be willing to mark such facts as do not coincide with their already received systems; all I mean is, that a preconceived idea often unfits us to attend to those facts even when they present themselves, and incapacitates us from finding them when we have to seek for them. In the description given, those facts only are mentioned which might, with good reason, be expected in the statements of a zealous supporter of the prevailing party. Those which have been omitted are of such a nature as, I think, would render it impossible to reconcile them to such statements.

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* Our author uses the term volcanic in preference to plutonic.

We shall now proceed to state the facts which, with the greatest force, and in the most striking manner, seem to require the rejection of the volcanic theory as altogether insufficient to explain the existing phenomena. Then, if we should have the good fortune to obtain a clearer view of matters, we shall, as far as we can, seek for a more perfect delineation of the whole subject.

Let us first revert to the position of the strata in our territory. Here we are told, that extraordinary disturbances in this respect have taken place. Highly inclined strata are not found generally in the porphyry districts, where—for it must be thus expressed in volcanic language—the molten masses must have flowed out *horizontally over* the strata; but still an elevation of the beds is in general to be seen in every part of the granitic districts, the masses of which have, on the whole, a more or less *vertical* position at the side of the broken-up bed. Here, only half of the truth is explained, for what relates to the facts, and what is implied in the mode of expression, involve an explanation totally false, which leads us to an equally false representation of the actual phenomena. Meanwhile, it cannot be denied that it is just by such anticipatory expressions, sometimes carried to a great length, that it is now pretty common to describe the geological relations in question. But now to the correction of the fault. It is true that a steep dip is a common phenomenon in the district; but it is also true that those localities only, where porphyry covers the strata, form a pretty constant exception to this, so that, in all other places where we are somewhat near one or other granitic boundary, more or less considerable dips will be regularly found. But to observe this is far from enough. Many other extremely important things are still to be determined. If we glance over the geological map, where all the irregular outlines, with their salient and re-entering bendings, are of granite masses, and observe that these masses lie sometimes longitudinally, sometimes transversely, so that they cannot be traced to an eruptive fissure running in one particular direction—if all this be remarked, then, according to the volcanic theory, there can be no regularity, either in the dip or direction. It may be taken for granted that the supporters of that theory, after having made their observations in about twenty places, and found nothing in the least corresponding with their ideas, would give up this trivial labour, and that then, as I suppose, they will remain satisfied with this brief conclusion, that the strata here were broken up by lava; and they naturally ascribe the appearances presented to the intrusion of abnormal formations, and especially to granite, in comparison with which, the other penetrating masses are of little or no consequence. As to myself, in consequence of the geognostical constitution of my country, I felt inclined to call in question the eruptive ideas, and I was stimulated to examine, in the most rigid manner, the direction and dip, in order to ascertain, if possible, if there really existed any causal connection between them and the massive formations occurring in the territory. Three of my former pupils have faithfully assisted me in this extensive labour; and we traversed, two and two together, the districts of the tract of country where the stratified rocks are not interfered with by the massive formations. We followed a strict method, almost as if we had been performing the duties of a land-surveyor; and, as soon as we had made all our observations, we brought them together for comparison. The results were truly surprising, and were, I think, of the highest importance, theoretically considered. The following is what possesses most interest for us at present:—In somewhat more than the whole northern half of the Christiania territory, there prevails in the stratified districts, disregarding a number of local irregularities, a law with regard to the position of the strata that cannot be mistaken, according to which, the dip is commonly above 45° nearly N. NW., a result which also agrees with that obtained in the Upland transition territory lying immediately to the north. Deviations from this law are found with equal frequency at points most remote from the boundaries of the massive rocks, as in their immediate neighbourhood, and our observation proves, in a manner which cannot be controverted, that these rocks could not have had any influence on the position of the strata, but that, on the contrary, this position, with all its irregularities, as well as its regularity, in short, in every respect as it now exists, *was produced before the formations spoken of were present in the territory.* This is now to be a little more exactly explained. The map shews that the stratified districts for the most part only occur as very narrow stripes around the massive districts, and are partly separated from one another. It must have been a very remarkable accident, and one which was not to be looked for, that these now separate portions of what must undoubtedly at one time have been a continuous whole—with regard to the earlier connection of which we are at one with the eruptionists—should in this manner have a conformable position of strata, if that had not existed before the separation. Let us particularly attend to the stripes running north and south; here the strata are transverse, and therefore quite short; they are thrust against the massive granite, are cut off by it, and still preserve their usual dip and direction. When we remember the parallelism already mentioned with the strata in the other territory, which has certainly been separated from the Christiania territory only by denudations of their common basis; upon which these massive mountain rocks could not act; and which, indeed, contains almost none of such masses, it seems evident enough that the arrangement of the strata here is a phenomenon which in its cause is altogether independent of the previous appearance of the granitic formations at these places (for it is only the granite, as already remarked, that we have here to take into our consideration). Still we will not pass over any fact which tends to strengthen a result so rich in consequences. Although the stratified districts are comparatively much less considerable in extent than the granitic, and remarkably so in regard to their narrowness, yet the most of them extend more than twenty-eight square miles (four square Norwegian miles), which perhaps with some, may lessen the weight of what has been adduced. It must then be remarked, that much smaller portions, either partially or entirely separated from the originally connected tracts of clayslate and limestone, always exhibit the same direction and dip which exist in the whole. The granite masses present deep sinuosities, into which long tongue-formed portions of the slate enter, or the same peninsula-like separation takes place by means of large offsets from the granite which enclose portions of slate between them; in either case, there is no disturbance to be traced in the general arrangement; finally, we have examples of complete separations, where also no change presents itself in the disjoined portions. This last circumstance is the most striking of the whole. In the middle of the great granite district to the north of Christiania, lies one of these entirely isolated portions of slate and limestone, which has a diameter of about one mile (one-fourth of a Norwegian mile). Here there is no clayslate, but in place of it only siliceous slate; and instead of limestone, there is a light coloured granular marble. Notwithstanding the whole strata have undergone the ordinary chemical alterations from their proximity to the granite, their position is perfectly undisturbed, inasmuch as they exhibit the usual dip and direction. Next, after this greatest specimen of completely separated stratified masses, I have seen smaller and smaller portions, down to pieces as small as the imbedded fragments of a conglomerate; and in all these this striking parallelism of the strata is recognisable.* Some of the observations on this subject were already published in 1826; and a celebrated German Professor said to me plainly at that time, that there must be some mistake in the observation, in short, that such a state of things was not possible; and truly it can hardly be reconciled with some of the theories which have hitherto at one time or another been generally received. I so much the more, on this account, invite geologists to investigate this fact for themselves; and convenient localities are to be found without much difficulty near Christiania, where such appearances are to be seen.*

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* Do not the sandstone masses enclosed in the basalt of the *Blaue Kuppe* at Eschwege exhibit the same relation? I infer this from a remark by Zeuschner, in Leonhard's "Neues Jahrbuch," 1835, p. 648. The portions of greywacke occurring in the Hartz granite, and the fragments of stratified

Let us now for the moment leave these phenomena, and pass to considerations of a different description, which are also among the most important connected with our territory, on account of the inferences they furnish. I allude to certain transitions which take place among some of the mountain-rocks.

(*To be continued.*)

Mr Adie's Meteorological Table,

Containing the Mean of the Meteorological Observations made at Regent Terrace, Edinburgh for each month of the year 1837, Lat. 55° 57′, height of the barometer above the mean level of the Sea 246 feet.

Months.	THERMOMETER.		REGISTER.		BAROMETER.		Rain.
	Morn.	Even.	Min.	Max.	Morn.	Even.	
January	37.13	35.52	31.48	38.45	29.704	29.469	1.23
February	40.71	33.96	34.00	43.71	29.556	29.479	2.14
March .	37.00	33.48	29.45	40.10	29.465	29.819	1.28
April .	41.47	37.83	32.93	44.93	29.947	29.630	1.61
May . .	50.90	45.84	40.58	55.39	29.751	29.770	1.53
June .	59.53	55.07	48.23	63.77	29.791	29.809	2.86
July . .	63.23	57.61	52.65	66.58	29.767	29.766	4.54
August .	58.58	54.97	49.23	61.94	29.810	29.789	4.13
September	55.40	50.97	45.57	57.83	29.693	29.719	1.73
October	52.48	48.87	43.45	55.00	29.687	29.746	2.02
November	42.03	38.87	35.00	43.13	29.458	29.482	2.03
December	43.39	41.97	36.55	45.39	29.694	29.705	1.67
Annual Mean,	46.53		45.64		29.688		Sum 26.77

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rocks in massive formations between Dresden and Meissen, deserve specially to be examined in the same point of view.

* The section of Tor-nid-neon at the head of Loch Ranza, in the island of Arran, may be mentioned as one of many instances in Scotland where masses resembling fragments of slate in granite have the same position as the bounding strata of slate.—EDIT.

Lord Gray's Meteorological Table for 1837.

Extracted from the Register kept at Kinfauns Castle, North Britain.
Lat. 56° 23′ 30″.—Above the level of the Sea 150 feet.

1837.	Morning, ½ past 9.		Evening ½ past 8.		Mean Temp. by Six's Therm.	Depth of Rain in Garden.	No. of Days.	
	Mean height of		Mean height of				Rain or Snow.	Fair.
	Barom.	Therm.	Barom.	Therm.				
January .	29.638	36.678	29.622	35.580	35.839	1.70	12	19
February	29.456	39.750	29.450	38.428	38.607	3.40	19	9
March .	29.747	37.71[illegible]	29.754	34.968	35.613	.95	5	26
April .	29.713	42.400	29.704	38.866	39.566	2.35	14	16
May . .	29.787	49.677	29.809	46.065	46.484	1.85	12	19
June . .	29.814	58.200	29.822	55.166	55.700	2.45	5	25
July . .	29.753	6[illegible].129	29.743	58.129	59.613	5.20	17	14
August .	29.801	57.161	29.791	54.677	54.839	2.80	10	21
September	29.705	53.066	29.748	47.533	50.466	1.33	9	21
October .	29.768	49.226	29.778	45.258	47.645	2.05	7	24
November	29.540	40.700	29.593	36.566	39.133	1.55	12	18
December	29.767	43.516	29.720	42.258	42.580	2.17	18	13
Average of the year	29.707	47.434	29.711	44.458	45.507	27.80	140	225

ANNUAL RESULTS.

MORNING.

	Barometer.	Thermometer.
Observations.	*Wind.*	*Wind.*
Highest, .	13th October,—— W. 30.50	24th June,——S.W.——70°
Lowest, .	13th February, —S.E. 23.60	1st January, N.W. ——23°

EVENING.

Highest, .	13th October,—N.W. 30.51	23d June,——S.W.——68°
Lowest, .	11th February, S.E. 30.40	11th January, N.W.——20°

Weather.	Days.
Fair, ...	225
Rain or snow, ...	140
	365

Wind.	Times.
N. and N.E. . .	30
E. and S.E. . .	86
S. and S.W. . .	124
W. and N.W. . .	125
	365

Extreme Cold and Heat by Six's Thermometer.

Coldest, . .	12th January,——————	Wind W. . . .	15°
Hottest, . .	24th June,——————	Do. S.W.	74°
Mean temperature for the year 1837, ...			45·507

Results of Two Rain Gauges.

	In 100ths.
1. *Centre of* Kinfauns Garden, about 20 feet above the level of the sea,	27.80
2. Square Tower, Kinfauns Castle, 180 feet, ...	27.49

Abstract of a Meteorological Journal for the year 1837, *kept at Anderson's Institution, Elgin, in Latitude* 57° 38′ *N.*, 3° 16′ *W.* By Mr ALLAN.

MONTHS.	9 o'clock, A. M.			3 o'clock, P. M.			9 o'clock A. M.	DIRECTION OF THE WIND, AT 9 O'CLOCK, A. M., AND THE NUMBER OF DAYS IT BLEW IN EACH DIRECTION.										
	BAROM.	THERMOM.		BAROM.	THERMOM.		Rain-Gauge.	S.W.	S.E.	N.W.	S.	W.	N.E.	E.	S.SW.	S.SE.	N.	N.NE.
		Inner.	Outer.		Inner.	Outer.												
January .	29.700	38°.5	37°.8	29.685	38°.6	38°.9	.290	12	3	3	3	6	2	1	...	...	1	...
February .	29.507	39.9	40.0	29.483	40.6	41.8	1.695	16	2	3	5	1	...	...	1	...	...	...
March .	29.805	37.8	36.7	29.765	38.4	38.9	1.340	10	1	7	...	6	3	2	...	2	...	...
April .	29.668	41.0	40.4	29.640	41.8	41.1	.770	8	5	4	2	3	4	3	1	...	...	...
May . .	29.754	49.3	50.9	29.767	50.2	52.2	.525	12	2	7	2	2	3	1	...	...	1	1
June . .	29.780	57.9	59.4	29.797	58.6	60.7	1.280	8	3	6	2	1	7	...	...	1	2	...
July . .	29.765	61.2	62.0	29.769	61.9	62.5	5.310	11	1	6	1	3	5	...	1	2	1	...
August .	29.805	59.0	59.9	29.782	59.6	60.3	3.630	13	9	5	...	2	1	1	...	0	...	...
September	29.741	53.5	54.5	29.676	54.6	56.2	2.562	10	6	2	6	1	2	1	1	1	...	...
October .	29.383	51.4	50.4	29.712	51.8	52.1	2.234	17	6	2	4	1	...	...	1	...	...	...
November	29.075	41.0	41.2	29.102	41.4	41.8	1.340	9	3	...	8	7	1	...	2	...	...	...
December	29.568	42.2	40.0	29.521	42.5	41.0	1.890	9	10	3	8	...	...	...	1	...	...	...
Mean .	29.628	47.7	47.7	29.641	48.3	48.9	22.866 (Sum)	135	51	48	41	33	28	9	8	6	5	1

The *Barometer*, the *Inner* and *Outer Thermometers*, used for the foregoing Journal of the Weather, are situated in a window fronting the north; and the observations are always taken at times when the sun's rays cannot affect them directly. The *Rain-Gauge* is raised about seven feet above the foundation of the building, and is at such a distance from it, that the rain is not impeded from falling into it from whatever quarter it comes. The average pressure of the atmosphere for the last 12 months is 29.634; the average internal temperature 48°; and external temperature 48°.3. The whole amount of rain is 22.866 inches, making a monthly average of 1.905 inches, being about 7 inches less in the aggregate than in 1836. The month of November of last year was 3° warmer than it was in 1836, and December was 4° warmer than the corresponding month of 1836. The S.W., the warmest wind, has increased 11 days above the year 1836, and 44 days above the year 1835. By these results, it is to be observed, as on former occasions, that the climate of the centre of Morayshire is exceedingly mild, as compared with the climate in the several surrounding counties, in which Registers are kept. And London, about 6° south of us, exceeds us in average warmth only by 1° and 7-10th deg.

Summary of Observations of the Barometer, Thermometer, &c. for the year 1837, *made at the Manse of the Parish of Abbey St Bathans, Berwickshire, Lat.* 55° 52′ *N. Long.,* 2° 23′ *W., at the height of about* 450 *Feet above the Sea.* By the Rev. John Wallace.

1837.	Thermometer.						Hygrometer (Leslie's).						Barometer (at 32° Fahrenheit).						Temperature of Spring Water.	Mean Point of Deposition.	Grains of moisture in a cubic inch of air.	Relative humidity.	Rain in Inches.
Months.	At 10 A.M.	At 10 P.M.	Means.	At 9 A.M.	At 3 P.M.	Means.	At 10 A.M.	At 10 P.M.	Means.	At 9 A.M.	At 3 P.M.	Means.	At 10 A.M.	At 10 P.M.	Means.	At 9 A.M.	At 3 P.M.	Means.					
January,	34°.5	33°.6	34°.0	33°.5	35°.4	34°.4	2°.	2°.	2°.	2°.	3°.	2°.5	29.371	29.361	29.366	29.369	29.349	29.359	46°	32°.6	.00136	951	2.673
February,	37.9	36.4	37.1	36.7	40.5	38.6	3.	2.	2.5	2.	5.	3.5	29.234	29.213	29.223	29.237	28.185	29.211	46	35.6	.00154	980	1.949
March,	34.9	30.3	32.6	33.1	37.	35.	6.	1.	3.5	3.	7.	5.	29.444	29.446	29.445	29.439	29.413	29.426	46	29.1	.00125	919	2.548
April,	39.5	35.0	37.2	38.3	40.7	39.5	8.	2.	5.	6.	10.	8.	29.271	29.273	29.272	29.266	29.273	29.269	46	34.	.00142	898	3.771
May,	48.4	42.3	45.3	47.	50.7	48.8	20.	5.	12.5	16.	23.	19.5	29.417	29.430	29.423	29.414	29.407	29.410	46	38.7	.00168	807	1.734
June,	58.2	49.7	53.9	56.7	59.2	57.9	20.	3.	11.5	17.	22.	19.5	29.443	29.452	29.447	29.439	29.440	29.439	46	49.5	.00237	865	2.247
July,	60.3	53.7	57.	58.7	61.4	60.	14.	1.	7.5	11.	17.	14.	29.400	29.397	29.398	29.400	29.385	29.392	46	54.3	.00278	925	5.450
August,	57.6	51.4	54.5	56.2	58.9	57.5	13.	1.	7.	12.	17.	14.5	29.444	29.444	29.444	29.446	29.438	29.442	46	51.9	.00257	921	3.376
September,	53.1	47.3	50.2	51.8	53.5	52.6	6.	1.	3.5	5.	9.	7.	29.361	29.372	29.366	29.360	29.359	29.359	46	48.5	.00230	946	1.952
October,	50.3	45.0	47.6	48.8	50.3	49.5	6.	1.	3.5	4.	9.	6.5	29.404	29.409	29.406	29.403	29.394	29.398	46	46.1	.00212	950	1.691
November,	39.8	36.8	38.3	38.1	40.4	39.3	3.	2.	2.5	1.	4.	2.5	29.110	29.140	29.125	29.104	29.105	29.104	46	36.8	.00156	957	1.506
December,	39.9	39.3	39.6	39.1	40.4	39.7	0.5	0.3	0.4	0.4	0.3	0.3	29.318	29.292	29.305	29.321	29.254	29.287	46	39.4	.00170	994	2.925
Means,	46.2	41.7	43.9	44.8	47.3	46.0	8.4	1.8	5.1	6.6	10.5	8.5	29.351	29.352	29.351	29.349	29.333	29.341	46	41.4	.00189	926	
																			Amount for the year,				31.822

Meteorological Table for St Andrews in Fifeshire, from 1821 *to* 1828.

The following table was constructed with much care and accuracy by the late Dr Jackson, Professor of Natural Philosophy in the University of St Andrews, from observations of the thermometer for eight years, at 10 A. M. and 10 P. M. daily, at a station elevated about seventy-eight feet above the mean level of the sea, and with a northern exposure, will afford a correct view of the mean temperature of the several months of the year at St Andrews, in latitude 56° 20′ 30″ north, and longitude 2° 47′ 30″ west.

	1821.	1822.	1823.	1824.	1825.	1826.	1827.	1828.	Mean of 8 years.
Jan.	37°.851	39°.974	35°.632	40°.839	30°.072	34°.845	36°.707	40°.871	37°.099
Feb.	39.375	40.994	34.564	40.284	39.500	41.982	35.531	40.562	39.099
March,	40.987	43.456	39.919	39.741	41.750	42.346	40.734	44.269	41.650
April,	47.041	46.525	43.876	46.341	47.683	48.126	46.058	46.342	46.499
May,	47.603	52.097	51.562	51.161	49.471	52.892	51.585	53.028	51.175
June,	52.740	58.642	53.758	54.467	57.323	64.063	57.780	59.837	57.326
July,	57.863	58.681	57.366	60.331	61.174	64.766	60.679	60.596	60.182
August,	58.608	58.348	56.705	57.645	60.508	62.295	58.437	60.712	59.157
Sept.	56.716	52.276	53.670	55.077	58.216	56.267	56.620	57.244	55.761
Oct.	49.255	48.585	46.621	47.281	50.438	50.403	52.484	50.172	49.409
Nov.	42.665	44.463	45.980	40.989	39.467	39.845	43.612	46.396	42.927
Dec.	40.153	37.619	38.369	38.395	39.419	40.817	42.662	44.253	40.209
Mean an. temp.	47.571	48.473	46.502	4 .712	47.918	49.887	48.574	50.357	48.374

The last number in the table, viz. 48°.374, is the mean annual temperature taken as the mean of the mean monthly temperature for the above eight years.*

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* From Statistical Account of St Andrews, about to appear.

An Attempt to ascertain Characters of the Botanical Alliances.

By Sir Edward Ffrench Bromhead, Bart. M. A., F. R. S. L. & E. (Communicated by the Author.)

Dr Lindley's celebrated "Nixus Plantarum" was the first attempt made in this country to characterise natural assemblages of the botanical families; but his object was to indicate the Normal state, and to offer differential characters, without a detailed examination of the whole structure. The work was one of uncommon boldness in the present state of the science; it did not enter into detail even as far as Agardh's Aphorismi, but fully entered into the spirit of that most original production. In Bartling is found the example, which must be followed, where we wish to examine the whole structure; his descriptive characters being laboriously worked out through every organ. Even Bartling, however, often gives only the Nixus, without indicating exceptions or equivalent structures, or applying language sufficiently general to embrace the whole conditions. The following sketches are too much open to the same remark; nor can accuracy and rigour be expected without the labours of a long succession of botanists. I have freely used the labours of the authors whom I have named, as far as they were applicable.

CHARACTERS OF THE ULVACEOUS RACE.

1. Ulvales.
2. Charales.
3. Osmundales.
4. Ephedrales.
5. Ulmales.
6. Urticales.
7. Piperales.
8. Haloragales.
9. Œnotherales.
10. Myrtales.
11. Rosales.
12. Saxifragales.
13. Cucurbitales.
14. Portulacales.
15. Chenopodiales.
16. Polemoniales.
17. Boraginales.
18. Solanales.
19. Gentianales.
20. Apocynales.
21. Galiales.
22. Cornales.
23. Geraniales.
24. Cistales.
25. Brassicales.
26. Nymphæales.
27. Sarraceniales.
28. Alismales.
29. Avenales.
30. Typhales.

1. Ulvales.—Without a matrix; evascular, surface without stomata or epidermis; of filamentous structure, filaments single or conglutinate and laminose; without a distinct axis of

vegetation, without true leafy appendages; colour a non-herbaceous green or olivaceous or purple. Not reproductive out of water, esexual, thecæ (if present) not regularly dehiscing, germinating by threads. None are poisonous.

2. Charales.—Articulate, ramose or sheathed; more or less tubular or ductulose, branches (if any) verticillate, often organizing earthy matter on the surface or transparent, not in definite gelatine, without leafy appendages, acrogenous. Acotyledonous, reproductive bodies of two kinds on the same or different plants, germinating by confervoideous threads. Inclining to a temperate habitat.

3. Osmundales.—Ductulose; stipe hollow; dichotomously veined; acrogenous. Esexual, phyllocarpous. Sporules indefinite and often angular. Inclining to a mild insular habitat.

4. Ephedrales.—Arboreous or frutescent, unigemmate or articulately branched; *leaves* (if any) pinnate or pinnately veined, without branched venation, exstipulate, simple. *Sexes* not united; *Inflorescence* in cones or aments or spikes or heads or solitary. Floral *envelopes* (if any) more or less persistent. *Corolla* 0. *Ovule* 1, erect, more or less gymnospermous or exalbuminous (Casuarinaceæ). *Albumen* (if any) enclosing the embryo; *embryo* straight; *radicle* turned from the hilum.

Inclining to a tropical habitat.

5. Ulmales.—*Leaves* alternate, simple. *Stipules* present (exc. some Myricaceæ). *Calyx* (if present) scarcely herbaceous, imbricate, persistent. *Petals* 0. *Stamens* definite, not incurved. *Carpels* 1 or 2, free from the calyx, forming independent cells, connate with each other. *Stigmas* divided or dilated or lateral. *Fruit* without a cupule, drupaceous or nucamentaceous or with a tough separable endocarp. *Ovules* 1–2 to each carpel. *Albumen* (if present) enclosing the embryo, not connate with the radicle.

6. Urticales.—*Foliation* passing from opposite and simple and entire to scattered and divided; *stipules* (if any) deciduous or convolute in vernation. *Sexes* not united; *Inflorescence* not amentaceous, flowers small. *Calyx* present, lobed or otherwise divided, seldom herbaceous. *Petals* 0. *Stamens* adherent to the calyx or in its centre, not adelphous; anthers bilocular. *Carpels* 1–5, connate with each other. Fruit with-

out a cupule, more or less indehiscent (? Hensloviaceæ). *Ovules* not more than one to each carpel or indefinite.

7. Piperales.—Not truly arboreous; *leaves* (if any) simple, stipulate or sheathing or opposite or decurrent. Male *flowers* small, spadiceous or amentaceous or spiked or solitary, subtended by a scale or spatha or bractea. *Calyx* (if present) of distinct sepals or narrow divisions. *Stamens* usually close to the germen. *Carpels* 1–4. As many *styles* or *stigmas* as carpels perfectly or imperfectly developed; stigmas often sessile or recurved or on setaceous or subulate styles. *Ovules* 1 to each stigma or indefinite. The *seed* or the *embryo* minute. *Albumen* present (exc. ? Podostemaceæ), embryo inclosed in albumen or in a vitelline sac (exc. ? Podostemaceæ).

Inclining to a warm habitat.

8. Haloragales.—Herbaceous or frutescent, *leaves* not compound though sometimes much divided; *stipules* 0. *Flowers* small. *Petals* (if present) fugacious, as many as the calycine divisions, alternating. *Stamens* not more than double the calycine lobes, not adelphous, inserted with the petals, adherent to the calyx (if any). *Carpels* 1–4, forming independent cells, connate with each other, adherent to the calyx (if any) or enclosed (Ceratophylleæ); pericarp not valved. *Ovules* not more than 1 for each carpel, pendulous, exarillate. *Albumen* (if present) fleshy and thin, enclosing the embryo; *embryo* only slightly or not at all curved; *radicle* to the hilum.

Inclining to a temperate habitat.

9. Œnotherales.—Not lactescent; *leaves* simple, penninerved, undivided. *Sepals* more or less connate, valvate in the bud or rarely somewhat open or in broken series (Vochyaceæ), odd sepal superior. *Petals* (when present) adherent to the calyx, not exceeding the number of calycine lobes, alternating. *Stamens* not more than four times the number of calycine lobes, adherent to the calyx, not adelphous. *Carpels* 1–4, not exceeding the number of calycine lobes, forming independent cells, connate with each other, adherent to the calyx or enclosed. *Style* 1, slender. *Ovules* indefinite and central, or definite and then pendulous or erect. *Albumen* 0 (exc. Montinia); *embryo* straight.

Inclining to a warm habitat.

10. Myrtales.—Not lactescent; *leaves* simple; *stipules* 0, or small and deciduous (Lecythidaceæ). *Sexes* united; flowers regular. *Petals* (if present) as many as the calycine lobes, issuing from the throat. *Disk* adherent to the calyx. *Stamens* perigynous, more or less inflexed in the bud. *Carpels* connate with each other, forming independent cells, more or less adherent to the calyx. *Style* 1; *stigma* undivided. *Placentæ* not parietal. *Albumen* 0 or brittle (Alangiaceæ).

Inclining to a tropical habitat.

11. Rosales.—*Leaves* alternate; the compound leaves *stipulate* but not compoundly stipulate, the simple leaves stipulate (exc. Lowea and some Spiræaceæ); not cirrhose. *Calyx* of 5 lobes or sepals, in an unbroken series, reflex or deciduous, the odd one superior. *Petals* rarely absent, as many as the calycine divisions, alternating, not connate with each other, issuing from the calyx, imbricated, not papilionaceous. *Stamens* incurved in æstivation, adherent to the calyx, not adelphous; *anthers* 2-celled, turned inwards (exc. Neillia), opening longitudinally (exc. Alchemilla arvensis). *Carpels* forming independent cells, not gynobaseose, without an epigynous disk, not connate with each other or rarely so under compression ~~of~~ by a fleshy calyx (some Pyraceæ). *Stigmas* not sessile; *styles* not connate. *Fruit* not leguminous. *Albumen* 0 (exc. Neillia, Hirtella); *embryo* straight.

Inclining to a mild habitat.

12. Saxifragales.—Not lactescent; *stipules* 0 (exc. Heuchera, Cunoniaceæ). *Sepals* 2–10, more or less connate, tubular at the base, more or less persistent (exc. some Cunoniaceæ). *Petals* rarely absent, not fewer than the calycine divisions, in a single row, alternating. *Stamens* not fewer than the calycine divisions, adherent to the calyx or issuing from a disk. *Carpels* 1–5, more or less connate with each other, more or less free from the calyx; carpels distinguishable early or at maturity by the diverging summits, or by the styles or stigmas or dehiscence. *Ovules* (if central) numerous and seeds small. *Albumen* fleshy, enclosing the embryo; *embryo* straight or nearly so; *radicle* to the hilum or superior; *cotyledons* short.

13. Cucurbitales.—Not arboreous; *leaves* simple; *stipules*

rarely present and then deciduous (Begoniaceæ); cirrhi (if any) axillary. *Corolla* (if present) not valvate. *Stamens* adherent to the calyx; *anthers* bilocular. *Carpels* 2 or more, connate with each other, more or less adherent to the calyx. *Stigmas* not sessile, or somewhat spiral (Begoniaceæ). *Placentæ* at the parietes, or on lamellæ proceeding from the centre towards the parietes. *Albumen* (if present) enclosing the embryo; *radicle* to the hilum.

Inclining to a warm habitat.

14. Portulacales.—Herbaceous or frutescent or rarely arbuscular (Fouquieraceæ), stems round or tetragonal; *leaves* simple and undivided (exc. Kalanchoe, Bryophyllum); habit succulent or nodes tumid; *stipules* 0 (exc. some Portulacaceæ). *Sexes* united (exc. some Silenaceæ, some Crassulaceæ), not affecting the floral envelopes. *Sepals* 2–20, more or less connate, persistent. *Petals* 3 or more or rarely absent (some Alsinaceæ, some Portulacaceæ), alternating with the calycine divisions when of the same number, more or less unguiculate or conniving at the base, not twisted (exc. Silenaceæ). *Stamens* attached to the calyx or to its base or enclosed in a tube, not fewer than the calycine divisions, not adelphous (exc. some Silenaceæ); *anthers* bilocular, dehiscing longitudinally and not outwards. *Carpels* forming independent cells in the bud (exc. some Silenaceæ); carpels more or less distinct, or having the styles (if any) more or less distinct (exc. Vivianieæ), or having the stigmas distinct, or dehiscing at the vertex. *Fruit* capsular or follicular (Crassulaceæ) or a berry (? some Silenaceæ). *Ovules* not parietal nor attached to the edges of imperfect dissepiments, seeds not comose. *Albumen* farinaceous or fleshy (Crassulaceæ, Fouquieraceæ ? Vivianieæ); *embryo* not enclosed in a vitelline sac, nor in the albumen except sometimes when fleshy (Crassulaceæ, Fouquieraceæ); embryo curved if exterior to albumen, or otherwise straight; *radicle* to the hilum (exc. Fouquieraceæ).

Inclining to a mild temperate habitat.

15. Chenopodiales.—Herbaceous or frutescent or rarely arboreous (Coccoloba), not lactescent; *leaves* simple, undivided (exc. some Chenopodiaceæ); *Stipules* 0, or minute and deciduous (Petiveriaceæ), or scarious (Illecebraceæ), or intra-

foliaceous (some Polygonaceæ). *Flowers* small (exc. some Nyctaginaceæ). *Sepals* 2–6, more or less connate or minute (Salvadoreæ), not valvate, rarely tubular and then indurated (Scleranthaceæ, Nyctaginaceæ), persistent. *Corolla* 0 or small and setaceous and imperfect (some Illecebraceæ) or membranous (Salvadoreæ), of as many divisions as the calyx. *Stamens* definite (exc. Phytolaccaceæ, some Petiveriaceæ), not adelphous (exc. Amarantaceæ). *Carpels* 1 or 3 or 5 or more (some Phytolaccaceæ), free from the calyx (exc. some Chenopodiaceæ), more or less connate with each other (exc. some Phytolaccaceæ, some Tetragoniaceæ). *Pericarp* simple; fruit a capsule or utricle or berry or caryopsis or nut or pyxidium, or membranous and dry and indehiscent. *Stigmas* not sessile (exc. some Amarantaceæ, Salvadoreæ). *Ovules* solitary and attached to the base with or without a funiculus, or numerous and attached to a central placenta. *Albumen* (if any) farinaceous or fleshy (some Phytolaccaceæ, some Persicariæ); *embryo* not enclosed in a vitelline sac, nor in the albumen except in some cases of apotropous radicle (Polygonaceæ); embryo bent or amygdaloid (exc. Petiveriaceæ, Polygonaceæ); *radicle* to the hilum (exc. Polygonaceæ).

Inclining to a temperate habitat.

16. POLEMONIALES.—Herbaceous or more or less frutescent, not lactescent; *stipules* 0. *Sexes* united. *Calyx* persistent. *Corolla* of 5 divisions (exc. some Hydroleaceæ), regular, monopetalous (exc. Staticeæ). *Stamens* 5, not adelphous; *anthers* 2-celled, not opening by pores. *Ovary* free from the calyx. *Styles* or their *stigmas* distinct. *Ovules* attached to the base with or without a funiculus, ascending or peltate. *Albumen* present, enclosing the embryo. *Embryo* straight, not antitropous.

17. BORAGINALES.—Not lactescent; *asperifoliate; stipules* 0. *Sexes* united; inflorescence circinate before flowering. *Calyx* herbaceous, monophyllous, persistent. *Corolla* of 4 or 5 divisions, hypogynous, monopetalous, not plaited, imbricate, deciduous. *Disk* more or less developed. *Stamens* as many as the corolline lobes, alternating, inserted on the tube, not adelphous; *anthers* bilocular, dehiscing by a longitudinal cleft.

Carpels 2, free from the calyx. *Style* 1. *Fruit* nucamentaceous or capsular (Hydrophyllaceæ).

18. Solanales. — Herbaceous or frutescent or parasitic; *leaves* rarely absent (Cuscutaceæ), alternate; *stipules* 0. *Calyx* of 4 or 5 divisions, odd one superior, persistent. *Corolla* hypogynous, of 4 or 5 divisions, monopetalous, not twisted. *Stamens* as many as the corolline divisions, alternating, adhering to the corolla. *Carpels* (if 2) having the midrib facing the odd sepal, free from the calyx, connate with each other (if not exceeding 4). *Seeds* (if definite) and erect or ascending, more or less albuminous, without a loose arilliform testa.

19. Gentianales.—Not arboreous, not lactescent, not aromatic, nodes perfect; *leaves* rarely absent (Leiphaimos), simple, entire; *stipules* 0 (exc. some Spigeliaceæ). *Sexes* united; *inflorescence* not gyrate. *Sepals* 4–8, connate with each other, persistent. *Corolla* hypogynous, of as many divisions as the calyx, alternating, monopetalous, not plicate. *Stamens* adhering to the corolla, not exceeding the corolline divisions, alternating; *anthers* bilocular; *pollen* usually of a ternary structure. *Carpels* 2, free from the calyx, the carpellary midrib alternate with the odd sepal, connate with each other. *Styles* more or less connate. *Seeds* small, without arillus. *Albumen* fleshy, enclosing the embryo; *embryo* straight; *radicle* to the hilum; *cotyledons* foliaceous in germination.

20. Apocynales.—Not aromatic, inclining to be knot-articulate; *leaves* rarely absent, not regularly alternate, simple, undivided, quite entire at the margin; *stipules* or cilia or glands interpetiolar (if any). *Sexes* united; *inflorescence* not gyrate. *Calyx* of 4–6 divisions, persistent. *Corolla* of 4–10 divisions, not fewer than those of the calyx, alternating when of the same number, monopetalous, not plicate, deciduous. *Stamens* adhering to the corolla, not exceeding the corolline divisions; *anthers* not unilocular. *Carpels* 2, the midrib alternate with the odd sepal. *Stigmas* more or less connate. *Placenta* at the suture when the carpels are distinct, or central when the carpels are connate, or free; *ovules* indefinite, or otherwise solitary. *Albumen* (if present) enclosing the embryo; *cotyledons* foliaceous in germination.

Inclining to a warm habitat.

21. Galiales. *Inflorescence* not gyrate. Odd calycine di-

vision superior. *Corolla* of 3 or 4 or 5 divisions, petals in æstivation connate or valvate or involute at the apex, deciduous. *Stamens* inserted on or with the corolla, not exceeding the corolline lobes, alternating, not adelphous; *anthers* ovate, bilocular, cells parallel, dehiscing longitudinally; *pollen* powdery. *Ovary* adherent to the calyx. *Stigmas* not sessile. *Placenta* not parietal. *Albumen* present, large, enclosing the embryo; *radicle* to the hilum.

22. CORNALES.—Not lactescent; *leaves* not dotted. *Flowers* regular. *Calycine* divisions (if any) not fewer than 4, sepals more or less connate. *Disk* developed. *Petals* inserted outside the disk, alternating with calycine divisions, more or less valvate in æstivation. *Stamens* not fewer than the petals. *Carpels* 1 or more and connate, forming independent cells, more or less adherent to the calyx (exc. Vitaceæ). *Fruit* baccate or drupaceous or coriaceous (Hamamelaceæ). *Ovules* 1 to each cell and pendulous, or 1 or 2 and basilar. *Albumen* enclosing the embryo; *radicle* to the hilum.

Inclining to a temperate habitat.

23. GERANIALES.—Herbaceous or suffrutescent; *leaves* not dotted. *Sexes* united. *Calyx* of 3–5 sepals or divisions, odd division superior. *Petals* 3–5. *Torus* without a pericarpous disk. *Stamens* not polyadelphous; *anthers* bilocular. *Carpels* 3–5, forming independent cells in the bud, free from the calyx, gynobaseose, more or less distinct by the styles or below (Limnanthaceæ). *Stigmas* as many as the carpels. *Ovules* attached to the suture or the inner angle. Inclining to a mild habitat.

24. CISTALES.—Frutescent or herbaceous; *leaves* usually simple, undivided, petioles 0 or short. *Stipules* 0 (exc. Elatinaceæ, Resedaceæ, and some Cistaceæ without amplexicaul petioles). *Flowers* solitary or racemose or cymose. *Sepals* 3–6, distinct or only shortly connate, odd sepal superior, in a broken series or irregular or more or less imbricate, persistent (exc. Krameria). *Petals* 3–6, hypogynous, more or less unguiculate or conniving at the base or irregular, not valvate. *Stamens* 3 or upwards, hypogynous; *anthers* innate or peltate or immoveable. *Carpels* 2–6 (? 10 some Cistaceæ), free from the calyx, connate to their summit (exc. Resedaceæ); disse-

piments (if any) vertical, formed of the inflexed edges of the carpellary leaves or of the endocarp issuing from the midrib. *Stigmas* not sessile (exc. Resedaceæ). *Ovules* 1 or 2 to each carpel or indefinite. *Fruit* a capsule, more or less dehiscent from the vertex (exc. Mundia, Monina, Securidaca, Krameria). *Albumen* present (exc. Tamaricaceæ, Krameria, some Linaceæ), enclosing the embryo.

25. BRASSICALES.—Not arboreous, nodes imperfect; *leaves* not opposite (exc. some Brassicaceæ); *stipules* 0 or simulated (some Cappareæ). *Flowers* not sessile, inclining to a binary structure, sexes united. *Calyx* of 2 or more sepals or divisions, deciduous. *Corolla* rarely absent, hypogynous, of 4 or more petals or divisions, deciduous. *Stamens* 2 or more, definite or in definite parcels or indefinitely quaternary, hypogynous. *Carpels* 2 or more, symmetrical, free from the calyx, connate with each other (exc. ? some Papaveraceæ). Dehiscence (if any) at the carpellary midrib; *placentæ* central to the valves; *ovules* many (exc. Tremandraceæ, some Brassicaceæ), seeds not erect. *Style* short or 0; *stigmas* alternating with the placentæ, obtuse. *Albumen* (if present) enclosing the embryo; exalbuminous embryo curved; *radicle* near the hilum; *cotyledons* foliaceous in germination.

Inclining to a temperate habitat.

26. NYMPHÆALES.—Herbaceous or frutescent, perennial, not climbing nor sarmentaceous; *leaves* radical or alternate, petioled, simple, usually large; *stipules* 0. *Sexes* united; *flowers* regular, not sessile, solitary on the scapes and large, or panicled and small (Cephalotaceæ). *Sepals* 3 or more; sepals and petals (if any) in one or more rows, same number (3–6) in each row; *petals* not twisted. *Stamens* not fewer than the row of sepals; rows of petals and stamens alternating; petals and stamens issuing from the torus, whether developed into a disk or not, and whether adherent to the calyx or ovary or neither; *anthers* bilocular, bursting longitudinally inwards, connective continuous with the filament. *Carpels* forming independent cells, free from the calyx, distinct or united by an urceolate cap (Nymphæaceæ). *Placentæ* basilar, or lateral, or attached to dissepiments proceeding from the parietes to the centre (Nymphæaceæ). *Albumen* (if present) having the em-

bryo near its base; albuminous embryo relatively small, exalbuminous embryo large or with thick cotyledons and highly developed plumule; embryo enclosed either in albumen or a vitelline sac.

Inclining to a mild habitat.

27. SARRACENIALES.—Not arboreous, not lactescent; *leaves* (if any) radical or alternate (exc. Clematideæ), petioled; *stipules* 0 (exc. some Ranunculaceæ, some Aristolochiaceæ). *Calycine* divisions 3–15, odd one superior. Having a *corolla* or corolloid perianth; *petals* (if any) not fewer than the calycine divisions, alternating, unguiculate. *Stamens* not fewer than the calycine divisions; *anthers* adnate or sessile. *Carpels* forming independent cells. *Seeds* or their embryo minute. *Albumen* enclosing the embryo near the base; *embryo* dicotyledonous before or after germination.

28. ALISMALES.—Aquatic or marsh plants, herbaceous. *Perianth* (if any) of 6 divisions, 3 or more petaloid. *Stamens* free from the perianth where the germen is free; *anthers* bilocular, turned inwards. *Stigmas* not sessile. *Seed-coat* not black. *Embryo* macropodal or with a scale-bearing plumule or inclosed in albumen.

29. AVENALES.—Not epiphytic nor parasitic; *leaves* narrow or pinnate or flabelliform, plaited in vernation or carinate; petiole or midrib sheathing. *Flowers* not large. Outer series of the *perianth* (if any) glumaceous or herbaceous, inner series petaloid. *Stamens* free from the perianth or attached only to the base. *Carpels* free from the perianth. *Stigmas* not sessile. *Ovules* definite, or indefinite and parietal. *Albumen* large; embryo small, not axile.

Inclining to a mild habitat.

30. TYPHALES.—Not lactescent, not bulbiferous; roots not calyptrate; *leaves* not fistular. *Inflorescence* more or less spadiceous or spicate or uniflorous. *Perianth* (if any) of 1–7 divisions, not petaloid or not wholly so. *Stamens* (where there is a perianth) definite and opposite the divisions, free from the perianth (exc. some Orontiaceæ). *Carpels* forming independent cells, more or less free from the perianth. *Ovules* 1 or 2 or indefinite, basilar or suspended or at the inner suture of the free or connate carpels. *Albumen* (if any) enclosing the

embryo, which is in such case cylindrical and straight and axile. *Embryo* (if exalbuminous) having a scale-bearing plumule or macropodal, with radicle turned from the hilum; embryo (in non-arboreous plants) having a lateral cleft for the emission of the plumule.

Inclining to a mild habitat.

Lincoln, June 1837.

(To be continued)

Explanation of the Table of the Natural Botanical Families.

A series of Families in immediate and continuous affinity with each other, is called an ALLIANCE, and is indicated by a termination in *ales* :—Ex. OSMUNDALES are the Ferns.

Parallel Alliances are called FORMATIONS, and are indicated by a termination in *osæ* :—Ex. LAMIOSÆ include Lamiales and Boraginales.

Series of successive alliances are called UNIONS :—Ex. The PASSIFLORAL UNION includes Violales, Passiflorales, and Homaliales.

Dr Lindley's Work on the Natural System is usually referred to for the Families and Tribes.

† Indicates that the Family or Tribe may be compound.

[ ] Indicates that the order of succession among the families so included is not settled

( ) Indicates that the evidence for the station is more conflicting than usual.

(1) Here separated.—(3) Link.—(4) D. Don.—(5) Jussieu.—(6) Bartling.—(7) Arnott.

Chenopodiaceæ include Scleranthaceæ-amarantaceae-†chenopodiaceæ.

Chondraceæ include Florideæ-thaumasieæ-gastrocarpeæ.

Cunoniaceæ include Caueraceæ-cunoniaceæ.

Diatomaceæ include Cymbelleæ-styllarieæ-fragillarieæ-desmideæ.

Digitalaceæ include (†) Verbasceæ-(†)digitaleæ-salpiglossideæ.

Fucaceæ include [Lichineæ-fucoideæ-laminarieæ-sporochnoideæ-chordarieæ-dictyoteæ-furcellarieæ-spongiocarpeæ].

Moraceæ include [Moreæ-ficeæ-dorstenieæ-daphnitideæ-artocarpeæ-(pouroumeæ)-chloropboreæ].

Nostocaceæ include Nostochineæ-rivularieæ-(batrachospermeæ).

Phytolaccaceæ include Tetragoniaceae-phytolaccaceæ-petiveriaceae.

Spiraeaceae include Spiraeae-quillaiae-(neillieae4).

Ulvaceæ include Caulerpeæ-ulvaceæ-(siphoneae-lemanieae).

Urticaceæ include [(Misandreæ)-(cecropieæ)-elatostemeæ-ureræ-boehmerieæ-parietarieæ-forskahleæ].

THE ULVACEOUS RACE.

[(†)Byssoideae, (†) fucaceae,[9] chondraceae,[9] (†)ULVACEÆ,[9] diatomaceae,[9] (†)nostocaceae,[9]]

Oscillatorieae-conferveae, ceramieae-ectocarpeae, CHARACEAE, equisetaceae, (sigillariaceae),
Ophioglossaceae, danæaceae, OSMUNDACEÆ, gleicheniaceae, polypodiaceae,
Cycadaceae, zamiaceae, EPHEDRACEÆ, gnetaceae, casuarinaceae,

†Myricaceæ, platanaceae-(†)moraceæ,[9] ULMACEÆ, stilaginaceae, scepaceæ,
Trewiaceæ, hensloviaceæ, (batideæ), (†)URTICACEAE,[9] cannabineae, datiscaceae,
Lacistemaceae, [chloranthaceae, garryaceae, (nageieae[2]),] PIPERACEAE-saururaceae, podostemaceae,

Ceratophylleae, hippurideae,[3] callitrichaceae, HALORAGEAE, trapaceæ,
Circæeae-†ŒNOTHERACEAE-montinieae, [lythraceæ, (†)rhizophoraceae, vochyaceae, combretaceae,
Memecylaceae, melastomaceae, alangiaceae, lecythidaceae-barringtonieæ, †MYRTACEÆ-puniceae,]

Pyraceae, amygdaleae, †chrysobalanaceae-†sanguisorbeae, [†potentilleae-ROSACEÆ, (†)spiræaceae,]

[Cunoniaceæ,[9] SAXIFRAGACEÆ, (†galacineæ,[4]), philadelphaceæ-aristoteleæ,] escalloniaceæ, bruniaceæ
Ribesiaceae, [†melocactaceae, CUCURBITACEÆ, begoniaceae, loasaceae,]
[(Fouquieraceæ), (crassulaceæ), mesembryanthaceæ,[9] PORTULACACEAE, vivianieæ-silenaceæ-†alsineæ]

(†)Illecebraceæ, †CHENOPODIACEÆ,[9] †phytolaccaceae,[9] †polygonaceae, mirabiliaceae-salvadoreæ,

Staticeae-plumbaginaceae, POLEMONIACEAE, cobæaceae, diapensiaceæ, hydroleaceae,
Hydrophyllaceae, BORAGINACEAE, heliotropiceae, ehretiaceae, cordiaceae,
Nolanaceae, [(†)digitalaceæ[9], SOLANACEAE-(cestraceæ,) convolvulaceae, cuscutaceae,]

Leiphaimaceæ,[2] [craufurdieæ-†GENTIANACEAE, menyantheae, retziaceae, spigeliaceae,
(†)APOCYNACEÆ, asclepiadaceae, carisseæ-rauwolfieæ,] potaliaceae-loganiaceae, lygodysodeaceae,
†Cinchonaceae, opercularineae, lonicereae-sambuceae, GALIACEÆ,] apiaceae,

(†)Araliaceae, [loranthaceae, (†)hamamelaceae, CORNACEAE-hederaceae,[6] †vitaceae,]
GERANIACEÆ, (surianaceæ), limnanthaceae, tropæoleae-balsaminaceae, †oxalidaceae,
[Reaumurieae-(tamaricaceae), elatinaceae-linaceae, CISTACEAE,] resedaceæ, polygalaceæ-kramerieæ,

Tremandraceæ, cappareae-cleomeae, BRASSICACEAE, fumarieae, papaveraceae,
[NYMPHAEACEÆ, nelumbiaceae, (†)cephalotaceae, hydropeltideae-podophylleae, pæonieae,[7]
Cimicifugeæ[7] -clematideae-†ranunculaceae, SARRACENIACEAE, aristolochiaceae, nepenthaceae,] * * *

(†)Pistiaceae, †hydrocharaceae, ALISMACEAE, butomaceae, pontederaceae,
Commelinaceae-philydreae, xyridaceae-restiaceae-desvauxiaceae, cyperaceae, AVENACEÆ, cocoaceae,
Cyclanthaceæ, pandaneæ-TYPHACEAE, (†)acoraceae-araceae-ambrosinieæ, triglochinaceae, †naiadaceae,

ERRATA.

Page 410, line 10 from bottom, *for* (Yochyaceæ), *read* (Vochyaceæ),
.. 411, line 18 from bottom, *for* compression of *read* compression by

Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, chiefly in the Royal Botanic Garden. By Dr Graham, Prof. of Botany.

Mar. 10. 1838.

Aristolochia saccata, *Wallich.*

A. *saccata;* volubilis; foliis oblongis vel ovato-oblongis, acuminatis, integris, subtus ramisque villosis; floribus lateralibus, racemosis; perianthio villoso, inflexo, medio ventricoso, limbo orbiculato angustissimo subrepando patentissimo, fauce maxima.—*Wall.*

Aristolochia saccata, *Wall.* Plantæ Rariores, 2. 2. t. 103.—*Ibid.* Catalogue of Indian Plants, No. 2707, *a.*—Bot. Mag. 3640.

Description.—*Shrub* volubile; *stems* very long, slender, branched; *bark*, when old, corky. *Leaves* (12–15 inches long, 4 broad) scattered, ovato-cordate, attenuated at the apex, slightly waved and sinuated, entire in the edges, petiolate, when young covered with brown hairs, which below are silky, more dense, subappressed, and longer than those above, where they are more erect; the leaves when old are less hairy, rather by the extension of the surface than by the hairs being deciduous; middle rib and primary veins prominent on both surfaces, transverse reticulations only prominent behind. *Racemes* arising from the stem near its base, where the leaves have dropped, and quite in the shade (even under the table in the stove) several flowered, pendulous, like the petiole and young branches densely covered with brown hairs, which are reflexed on the last, but erecto-adpressed on the first two. *Flowers* membranous, covered with spreading hairs, pendulous, tube turned upwards in the middle, the reflected portion being parallel to and in contact with the other, yellowish-white within and without, but having externally many nerves, smaller transverse reticulations, and small glandular excrescences, all of a red brick colour, contracted a little above its base, and below this, both within and without, of a dirty red colour, and having spread over its inner surface a covering of hairs singularly crystalline and ramified, forming a large pouch where it is reflected, and in its erect portion again contracted, flattened in front, and slightly clavate upwards, within this portion minutely glandular, but without any hairs; throat circular, placed vertically, of bright yellow colour, with a narrow erect margin; limb narrow, before expansion folded in three triangular divisions across the throat, when expanded flat and obscurely 3-lobed, on its upper (anterior) surface dark purple, densely covered with erect warts of similar colour, on its lower (posterior) surface similar to the outer surface of the tube. *Anthers* six, yellow, 2-celled, bursting along the front, oblong, sessile upon the short stout clavate *style;* pollen yellow, granules nearly globular. *Stigma* of three acute connivent lobes, sending projections downwards upon the style, so as to separate the stamens into pairs. *Germen* inferior, slightly clavate, furrowed, densely covered with erect brown hairs. *Ovules* very numerous, horizontal.

This curious plant is a native of Silhet, and was introduced into the Royal Botanic Garden, Edinburgh, from the Calcutta Garden, in 1829. It produced a succession of flowers in September last, but formed no fruit. I did not perceive that exceedingly offensive smell for which Dr Wallich, in the splendid work above quoted, says they are remarkable.

The ensnaring of insects by plants is observed in many cases, its use disputed, sometimes I think misunderstood, and its benevolence in the arrangement of nature considered equivocal. One thing is obvious,—it demonstrates premeditation and design in the configuration of parts. The large heavy pouch in the middle of the tube necessarily keeps the flower pendulous, and its throat erect. Having removed from the plant one of the racemes for examination, I laid this down on the table, and was surprised to see a crowd of small flies immediately rush out at the throat. I raised the flowers into their natural position again, and though I saw, by placing them between me and the light, that very many flies were still in the tube, all very restless, and attempting to escape, not one could climb up the now erect throat. I repeated this experiment many times, and always with the same result;—in the horizontal position of the flower the flies came out instantly,—in the erect position they were imprisoned. I could not discover with the microscope any cause for this, and am forced to suppose that there may be a particular condition of the surface in the upper part of the tube, from secretion, or other cause, which prevents the adhesion of the feet of the insects, though they are able to walk along it when horizontal.

It is supposed that the confinement of insects in flowers is to effect the impregnation of these; and it has been thought, that the decay of their bodies in other parts, as in *Dionæa*, *Nepenthes*, and *Saracenia*, tends to the nourishment of the plant. The first I believe is sometimes true; though I disbelieve the second, I have not, in every supposed instance, the means of disproving it. In the instance under consideration, and, I believe in others, the object seems altogether different. Years ago I observed a living worm in several of the decayed leaves of *Dionæa muscicipula*, and was induced in consequence to suspect that the capture of certain insects by this plant was not for their destruction, but to provide a proper nidus for their eggs; and I more confidently believe that this is the case in *Aristolochia saccata*, for in all the flowers of this plant which I opened, I found many perfect eggs, and many living maggots. Some insects wrap up their eggs in leaves; to others this instinct is denied, but protection is extended to their race by what, imperfectly understood, has been thought an act of unmixed cruelty.

Batemania Calleyi, *Lindl.* var.

Batemania.—Flowers ringent. Sepals spreading, the lateral ones equal, and attenuated at the base. Petals broader than the sepals, oblique at the base, adnate to the elongated base of the column. Labellum articulated with the column, 3-lobed, concave. Column half cylindrical, elongated at the base; clinandrium bordered. Anther small, bilocular (?) membranous. Pollen-masses 2, 2-lobed on the back, sessile, gland triangular.—*Lindley.*

Batemania Calleyi, *Lindl.* in Bot. Reg. 1714.

DESCRIPTION.—*Pseudo-bulbs* ovato-pyramidal, subtetragonous, corrugated, dark green, with a loose withered bivalvular sheath. *Leaves* 2, terminal, the outer the smaller, coriaceo-membranous, lanceolate, undulate, recurved, dark green above, paler below, having about nine nerves, which are prominent behind, and slightly channelled in front. *Racemes* several, from the base of the new and not yet enlarged pseudo-bulb, pendulous, many-flowered, zig-zag. *Flowers* pale rose-coloured, alternate, but opening towards one side, rising singly from the axils of large ovato-rhomboid cucullate bracteæ, pedicellate, the pedicels longer than the bracteæ. *Sepals* unequal, the two lateral (1 inch long) spathulate, narrow, projecting forward and downwards, divergent, the upper (three-fourths of an inch long) shorter, elliptical, connivent. *Petals* falcato-oblong, broader than the upper sepal, scarcely shorter than it. *Lip* equal in length to the petals, concave, 3-lobed, lobes denticulate, rounded, the lateral ones erect, the central recurved and longer, undulate, and marked longitudinally by elevated lines; disk fleshy, with a ragged lobe beyond its middle. *Column* half cylindrical, somewhat clavate, curved forward, with an undulate crenate wing round its apex, its base projecting forward, there adhering to the petals and lateral sepals, and articulated to the labellum at its point. *Stamen* terminal; anther-case nearly square, bordered, unilocular; pollen-masses 4, subsessile, pear-shaped, the two lower smaller; gland subulate, projecting into the centre of the flower. *Stigma* transversely oblong. *Germen* sulcate, not twisted, shorter than the pedicel or sepals.

The plant described flowered very freely in the stove of the Botanic Garden in December and January 1837–8, and, though it differs from that figured by Professor Lindley very greatly in its colour, and in the form of the lobe in the disk, can only be considered a variety of the same species. Possibly the colour may have been in some degree the consequence of the season at which the plant flowered.

Rhododendron albiflorum, *Hooker.*

R. *albiflorum*; fruticosum; foliis lanceolato-oblongis, utrinque ramulisque pilosis, subtus pallidis; pedunculis axillaribus, unifloris, solitariis binisve; calycibus 5-partitis, pedunculo longioribus, segmentis ellipticis; corollis campanulatis calycem duplo superantibus.

Rhododendron albiflorum, *Hook.* MS.

Description.—*Shrub* erect, branched from the root, bark dark brown, yellowish-green on the twigs, and there sparingly covered with long subappressed dark brown hairs. *Leaves* (1½ inch long ⅗ths of an inch broad) very shortly petiolate, lanceolato-oblong, crenulate, undulate, concave upwards, sparingly covered on both sides with hairs similar to those on the shoots, bright green above, and often coloured towards the edges, paler below where the middle rib and veins are prominent, but grooved on the upper side. *Peduncles* axillary, single-flowered, solitary or in pairs. *Calyx* 5-partite, longer than the peduncle, as well as it greenish-yellow, and covered with short green glandular hairs, and others which are longer, somewhat chaffy, brown at the base of the peduncle, but becoming greener upwards over the calyx; segments imbricated, equal. *Calyx* elliptical, shortly ciliated, glabrous within, nerved. *Corolla* (three-fourths of an inch across, nearly as long) campanulate, 5-cleft, white, with a few small orange-coloured circular spots on both sides of the upper segment, and on the upper side of the two adjoining segments, glabrous except on the throat, which is hairy, obscurely veined, twice as long as the calyx; segments imbricated, cordato-ovate, blunt, at the bottom of the corolla the sweet juice is collected into five globules, but there is no conspicuous nectary. *Stamens* ten, shorter than the corolla, rather longer than the calyx, nearly equal, the lower and the alternate ones rather longer, subequally connivent, hypogynous; filaments subclavate, hairy in their lower half; anthers elliptical, short, notched at both ends, attached to the filaments at the base of the upper notch, without awn, bilocular, opening by an oblique elliptical hole at the top of each loculament. *Pistil* nearly equal to the stamens in length; stigma of 5 erect, subdivided lobes, surrounded by a horizontal border; style clavate, glabrous in the upper half, hairy in the lower, slightly declined; germen rounded, in its upper half obscurely lobed, dark green, and covered with long glandular hairs, and short dense pubescence, in the lower half subglabrous without any of the glandular hairs, yellow, distinctly 10-lobed, 5-celled, each cell with a large placenta projecting from the middle of the central column, and covered with numerous ovules, the lower and upper part of the germen being empty.

This very distinct species was raised at the Botanic Garden from seed gathered by Mr Drummond in British America in 1828. It does not grow freely, and flowered rather sparingly in the open border for the first time in July 1837. It is to be regretted if it is found difficult of cultivation, for Mr Drummond stated that it formed a very handsome shrub.

Proceedings of the Royal Society.

1837, *December* 4.—Sir T. M. Brisbane, Bart., President, in the Chair. The following communications were read :—

1. On the Food of the Vendace, Herring and Salmon. By John Stark, Esq.

The author in this memoir gave a full account of the food of these species, as contained in the writings of authors, and as ascertained from actual observation; in answer to some lucubrations on this well known subject laid before the Society

December 18.—Dr Hope, V. P. in the Chair. The following communications were read :—

1. Remarks on the Ossiferous Caves of Cefn, in Denbighshire. By Professor Traill.

These caves, which were first described by the present Bishop of Norwich in 1832, and have since been more fully explored by Dr Cumming of Denbigh, were visited by the author in the autumn of 1837. The principal cave is a fissure in a grand mural escarpment of the mountain limestone of Wales, about two miles and a-half south-west of St Asaph, and occurs half-way down the precipice, which seems to be about 250 feet in height. It forms at that point the southern boundary of the limestone, which constitutes the basis of the Vale of Clwyd; and is divided from the extensive greywacke slate formation of that county by the narrow picturesque Vale of Cyffredin, through which the river Elwy flows.

The hill of Cefn consists of parallel beds of limestone, which the extensive quarries on its southern flank shew to have a regular dip of about 8°. This cave was discovered in 1830 to contain earthy deposits exceedingly rich in bones of mammifera; and, since that period, they have been much employed as a manure by Mr Lloyd, the proprietor. During the excavations for this purpose, many teeth and fragments of larger bones, so entire as to be readily recognised, have been obtained. An interesting collection of them is preserved at Cefn House, and some are in the hands of the author. Among the former, he noticed part of the humerus and a molar tooth of a rhinoceros, several teeth and bones of the hyæna, and beautiful teeth, and a considerable portion of the lower jaw of a bear. Dr Traill has in his possession two phalanges and two teeth of a bear; a phalanx of a large Felis, resembling the tiger; parts of the tibia, and of the astragalus, and a phalanx of a large Bos; portions of the metacarpus of an immense ruminant, apparently a deer; besides a variety of fragments, not so easily ascertained on account of their mutilated state.

The materials, which filled up the fissure or principal cave almost to its roof, are regularly stratified. They formed together a mass of earthy matter twelve feet in thickness. The first or upper

[illegible]

[illegible]

[illegible]

[illegible]

2. [illegible] Salmon [illegible] of seven [illegible]

[illegible] published in the present volume of [illegible]

January [illegible] —Dr Abercrombie, V. P. in the Chair.

The following communication was read:—

1. Terrestrial Mechanism of the Tides. By John Scott Russell, Esq.

[illegible] title Terrestrial Mechanism of the Tides, the author of this paper means to include the consideration of those causes which regulate the propagation of the tides, so as to exclude entirely the examination of the mechanism by which they are primarily produced. The generation of the tidal elevation in the Pacific or Atlantic Ocean is a question entirely of celestial mechanics. But the tides, after having been generated by solar and lunar attraction in a manner that is found to be perfectly in accordance with the varying intensities of these forces, do not subside at the instant when these forces cease to act, but continue to exist during a long period of time, reaching our shores three days after their birth; and have obeyed, during this long interval, laws perfectly independent of the original influence by which they were produced, and presented phenomena in direct opposition to it. They have obeyed the laws of terrestrial hydrodynamics. The law of the propagation of the tidal wave through the ocean and around our shores, belongs therefore to terrestrial, not to celestial mechanics.

Although our knowledge of the celestial mechanism of the tides has recently attained a high degree of accuracy, our knowledge of the terrestrial mechanism has been hitherto almost entirely conjectural. In the former department, the system of Bernouilli, based on the discoveries of Sir Isaac Newton, presents us with a theory of the tides in close accordance with the phenomena. Laplace, in his elaborate discussion of the tides of Brest, executed by M. Bonnard, has verified this accordance to a high degree of precision; and the recent researches on the tides, by Mr Lubbock and Mr Whewell, may be considered as having rendered the celestial mechanics of the tide as perfect as any other department of Astronomy, the errors of prediction being now reduced within the limits of the errors of observation; so that Mr Lubbock has stated that he does not look forward to any material improvement in this department of our knowledge.

But our acquaintance with the terrestrial mechanism of the tides is in a very different condition. In reference to the phenomena of this department, Laplace has said, in the third chapter of the fourth book of the Mecanique Celeste, and he repeats the opinion in the first chapter of the thirteenth book, at a much later date, where he says: " Que les circonstances accessoires produisent des variétés considerables dans les hauteurs et dans les heures des marées des ports même très rapprochés;" and then he adds, " Il est impossible de soumettre au calcul ces variétés, parceque les circonstances dont elles dependent, ne sont connues." Mr Lubbock, in like manner, speaking of the fluctuation of the establishment, says, " This perplexing fluctuation presents an insuperable obstacle to extreme accuracy in tide predictions, until it can be explained; at present we are only left to conjecture respecting the cause." And Mr Whewell says, in 1837, " I cannot conclude this paper without again pointing out that a great number of curious facts in fluid motion are established in these Tide Researches, of which it may be hoped the theory of hydrodynamics will one day be able to render a reason."

In 1834, the author of this paper had determined the law of the interference of a certain species of wave in the resistance of fluids, and this species of wave appeared to present very striking analogies to the tide-wave. In 1835, Mr Whewell expressed his decided conviction of their similarity, and Mr Robison and Mr Russell were, in 1836, appointed a Committee of the British Association to investigate the subject. The Report of these observations was given to the British Association in 1837; but as the observations themselves are not necessarily connected with the particular conclusions which may be drawn from them, these conclusions, at which the author had arrived, were made the subject of a separate paper to this Society.

The paper embraced three subjects of examination.

I. *Regarding the true nature of the tide wave.*—By observations made on the tides of the rivers Dee (in Cheshire) and Clyde, it was ascertained that the tidal elevation is a large compound wave consisting of a series of elements, each of which is identical with the wave of the first order, or great primary wave of translation, of which the author had already determined the nature and the laws. The *velocity* of the tide-wave is that due to the depth of the centre of gravity of the transverse section of the channel below the surface. The tide wave is a wave of translation, but the velocity of translation of the particles is entirely different from the velocity of propagation of the wave, the former being variable, and the latter constant. When the velocity of propagation is greatest, the velocity of translation is often least. The translation takes place almost uniformly to the greatest depth hitherto observed.

II. *The application of our knowledge to the explanation of the ordinary and extraordinary observed phenomena of the tide.*—The identity of the tide-wave with the wave of translation affords an explanation of the following phenomena, which have hitherto been wholly unexplained or imperfectly understood. (1.) The dislocation of the tide by which the time of ebb is increased, and the time of flood diminished by propagation, after a considerable space. (2.) The difference between the height of tides in places which are adjacent, and have been expected to have nearly the same tides. (3.) The variation of the establishment from time to time, arising from changes in the dimensions and form of the bottom of the sea or channels of rivers. (4.) The cause of the breaking surge or tide-bar. (5.) The variation in the form of the curve of semi-menstrual inequality in the height and in the interval at different places, and the exaggeration of the other inequalities.

III. *The deduction of practical maxims for the improvement of tidal channels.*—The last part of the paper is directed to the determination of the principles by which tidal channels may be improved, so as to facilitate the ascent of the tide,—to increase its volume,—to diminish the duration of ebb, and increase that of flood-tide,—and to impede the action of the tide in moving sand, producing bars, and injuring the banks of tidal channels or rivers.

Proceedings of the Wernerian Natural History Society.

(*Continued from p.* 209.)

THE thirty-first session of this Society commenced on the 25th November 1837; when the following gentlemen were elected office-bearers for 1838, viz.

President.

ROBERT JAMESON, Esq. F.R.SS.L. & E. Professor of Natural History in the University of Edinburgh.

Vice-Presidents.

Dr JOHN COLDSTREAM,
DAVID FALCONAR, Esq.
Dr CHARLES ANDERSON, M.R.C.S.
WILLIAM COPLAND, Esq. F.R.S.E.

Secretary.—Dr P. NEILL, F.R.S.E.
Assistant Sec.—T. J. TORRIE, Esq.
Treasurer.—A. G. ELLIS, Esq.
Librarian.—JAMES WILSON, Esq.
Painter.—P. SYME, Esq.
Assist. Painter.—W. H. TOWNSEND, Esq.

Council.

Dr R. K. GREVILLE, F.R.S.E.
JOHN SLIGO, Esq. F.R.S.E.
Dr WALTER ADAM, F.R.C.P.
Dr WILLIAM MACDONALD, F.R.S.E.
Dr MARTIN BARRY, F.R.S.E.
RO. JAMES HAY CUNNINGHAM, Esq.
W. A. CADELL, Esq. F.R.SS.L. & E.
Dr ROBERT HAMILTON, F.R.S.E.

At this meeting Dr Martin Barry exhibited a living specimen of the Proteus anguinus; and having, with a lancet, drawn a small portion of its blood, shewed the globules by means of a microscope by Schiek of Berlin, these globules being about fifteen times larger than those of human blood.

1837, *Dec.* 9.—Professor JAMESON P. in the Chair. Mr Torrie read the Rev. Dr Anderson's Account of remarkable Fossil Remains, especially Fishes, found in the sandstone of Fifeshire; and exhibited some interesting and beautiful specimens. Mr Torrie then read Dr Boué's remarks on the scenery, antiquities, population, agriculture, and commerce of Central Turkey, Part 1st (since published in the last number of this Journal, p. 121). Professor Jameson exhibited an enormous tibial bone of a Mammoth, from the base of the Himmalaya range in Upper India; likewise two skulls of the great or red Oran-outang; and a fine specimen of the Stork killed on the mainland of Shetland last autumn, and transmitted by William Mouat Cameron Mouat, Esq. of Garth.

Jan. 13. 1838. Professor JAMESON P. in the Chair—Mr Torrie read Mr G. Maclaine of Batava's Remarks on the Geology and Mineralogy of Java, which were illustrated by specimens; likewise Remarks on a species of the order Cheiroptera, taken in the tombs of the kings at Thebes, by Dr William Hibbert of the Queen's Royals; communicated by Sir James M'Grigor. Mr William Jameson exhibited and described various specimens of new or rare birds from Northern India.

Jan. 27.—DAVID FALCONAR, Esq. V.P. in the Chair.—Mr Torrie read a communication by Dr Charles Bell, Physician to the British Embassy at the Court of Persia, on the Geology of part of the district of Mazunderan. (This Paper has since been read at the Geological Society of London.) Professor Jameson exhibited and described a fine specimen of the head of a large Mastodon from India; from the collection of Colonel Colvin. Mr Kemp exhibited the experiment of the solidification of carbonic acid gas.

Feb. 10.—DAVID FALCONAR, Esq. V.P. in the Chair—Mr Torrie read, 1. Dr Boué's Remarks on the scenery, antiquities, population, agriculture, and commerce of Central Turkey. Part 2d (Published in our present Number, p. 237.) 2. A letter from Dr Smith of Lima on the use of ice in the cure of cholera morbus in Peru. 3. A letter from J. B. Pentland, Esq. containing notices on elevated beaches in South America, (an extract is published in the present Number). Mr William Jameson communicated some remarks on gulls, and described a new species from India. Mr Kemp shewed the action of potassium on various gases.

Feb. 24.—DAVID FALCONAR, Esq. V.P. in the Chair—Dr Martin Barry gave microscopic demonstrations of the ciliary motions, as well as of individual ciliæ, in the Ostrea edulis; also of an unimpregnated ovulum of the Lepus timidus, &c. and described a series of diagrams, illustrative of the various parts of the ovulum in all classes of animals, from the infusoria to man.

March 10.—Professor JAMESON, P. in the Chair—Dr Robert Hamilton exhibited beautiful coloured drawings of the known species of seals, and made remarks on the characters and habits of each. Mr R. J. Hay Cunningham read a paper on elevated beaches, in reference especially to a deposit near Cockburnspath. Dr Martin Barry then read a paper on the Blood, in regard particularly to the application of histological characters in zoological classification, first proposed by Professor Wagner of Erlangen in Bavaria. He pointed out some remarkable coincidences between the size of the red particles and the degree of concentration of the germinative spots in Fishes and Amphibia, and gave a microscopic demonstration of the blood-granules of the Ostrea edulis, and of the unimpregnated ovulum of Birds and osseous Fishes.

Botanical Society of Edinburgh.

FROM an early period in the history of the University of Edinburgh, the Professors have recommended and encouraged the formation, among the students, of philosophical and literary societies, where not only the different branches of science and literature, as taught in the University, but also the novel views and original investigations brought forward by the members, could be freely and amply discussed.

These societies have gradually increased in importance, and their usefulness has been much extended by the rich libraries and the museums attached to some of them.

The following list of these societies will afford to the public an opportunity of judging of the extent of this branch of our University system, which, highly as it is esteemed, is not so generally known as it deserves.

Societies formed by, and conducted under the management of, the Students in the University of Edinburgh.

Medical.—1. Royal Medical. 2. Hunterian. 3. Anatomical.

Natural History.—4. Royal Physical, which embraces General Natural History and Chemistry. 5. Plinian—General Natural History, and Natural History of Scotland. 6. Cuvierian—Zoology and Geology. 7. Botanical.

Natural Philosophy.—8. Physical and Mathematical.

Literature.—9. Speculative, and some minor societies.

We do not include in our list the Law and Theological Societies.

Having been requested by the Botanical Society to allow an abstract of their scientific proceedings to appear in this Journal, we do so with great pleasure, from knowing that this public record of its proceedings cannot but be useful to the Society, and also gratifying to the lovers of botany.

11*th January* 1838.—Professor Graham, President, in the Chair. Mr R. W. Falconer read a paper on "the ancient history of the Rose," in which he gave an account of the rose trees mentioned and described by the Greek and Roman writers of antiquity; also of the modes in which roses were cultivated, their periods of flowering, and the various uses to which they were applied. Theophrastus and Pliny appear to have given the fullest account of the rose, the former enumerating five kinds of roses, the latter fifteen, eleven of which, he says, were familiarly known to the Romans. After comparing the descriptions given by these authors with those of Dioscorides, Clusius, and other writers, Mr Falconer proceeded to give an account of the ancient rosaria or rose plantations, collected from the various works of Pliny, Columella, and Palladius; also of the means employed for propagating and forcing roses mentioned by Theophrastus, Didymus, Pliny, and Seneca. The different localities renowned for their roses were next stated; Nicander, Athenæus, and Pliny, being the principal authorities on this point. Among the ancients the rose was employed medicinally, at their festivals, and at their sacred ceremonies; also as an article of luxury at their banquets, and for making unguents. The uses of the rose among the Greeks and Romans were nearly the same, the latter nation, however, using them more profusely, and setting a higher value upon them. Anacreon was the first author whom Mr Falconer could find to have mentioned the rose, and he flourished about 600 B. C. Myrepsius, a medical writer of the 13th century, was the latest author quoted.

A communication from Mr Edwin Lees of Worcester was then read, giving an account of a specimen of *Pyrus domestica*, Sm., or Sorb-tree, now growing in Wyre Forest, Worcestershire. Mr Lees thinks it probable, from the situation of Wyre Forest, on the confines of three counties, Worcester, Salop, and an isolated portion of Stafford, that this locality for *Pyrus domestica* may have been inadvertently multiplied; and that the station given by Dr Plot and Ray, in the "moorlands of Staffordshire," possibly may refer to the specimen in question, which, however, is situated in the parish of Rock, in Worcestershire, about three miles from Bewdley. From a close inspection of the locality, Mr Lees is inclined to think that the tree alluded to is not there indigenous, although probably entitled to an antiquity of not less than 400 years. The vestiges of a habitation and garden he thought might be traced in some bricks and remains near the spot, and here he found solitary specimens of *Ligustrum vulgare* and *Prunus domestica*, the only ones which he observed in the whole forest. The tree, when visited in 1836, was much dilapidated, and presented the appearance of extreme old age in the battered state of its bole, great height, broken lower branches, and tenuity and tortuosity of the upper ones, which only bear flowers from the young shoots at their very ends. Fruit is produced annually, and is eagerly gathered as a curiosity by the country people, who look upon it as a charm, suspending it in their habitations, and appearing to consider it a safeguard, while to the mountain ash (*Pyrus Aucuparia*) they pay no sort of attention, although they seem to be fully aware of the relationship between the species, designating the latter the "Whitten tree," while the former is called the "Whitten pear-tree," the fruit very much resembling a small jennet pear. The stations given for *Pyrus domestica* in Cornwall and the Isle of Wight, Mr Lees thinks rest on doubtful authority, and that its claims to being considered indigenous to Britain would require to be based on a stronger foundation than that afforded by the solitary individual in Wyre Forest.

Dried specimens of plants were presented from many members of the society, received since 14th December last, along with various donations to the library.

SCIENTIFIC INTELLIGENCE.

PHYSIOLOGY.

1. *On the Gases contained in the Blood, and on Respiration.* By M. G. Magnus.—M. Magnus remarks, that it remains a question, whether carbonic acid is formed in the lungs by the oxidisement of a part of the carbon in the blood by the action of the air, or whether venous blood, when it reaches the organs of respiration, contains carbonic acid ready formed, which is merely separated from it? M. Magnus passed hydrogen gas through a solution of potash, to deprive the gas of any carbonic acid which it might contain, and when it gave no precipitate with lime-water, he passed it into the blood of a healthy man; the gas afterwards made to go through the lime-water, gave a plentiful precipitate of carbonate of lime. Azotic gas similarly employed, produced a like effect; and M. Magnus concludes, from these experiments, that carbonic acid exists ready formed in the blood, and consequently that it is not formed in the lungs. Carbonic acid was also separated from blood by means of the air-pump. By using Liebig's apparatus, M. Magnus found that blood contained about one-fifth of its volume of carbonic acid gas, and when it had been kept 24 hours, without emitting any bad smell, the quantity was larger. The results were confirmed by employing atmospheric air instead of hydrogen gas. M. Magnus then ascertained the nature and proportions of all the gaseous contents of the blood. He found that 100 volumes of the arterial blood of a horse yielded carbonic acid gas 4.32 volumes; oxygen, 1.52; azote, 2; total 7.84 volumes. The venous blood of the same horse, drawn four days afterwards, gave carbonic acid gas, 4.29 volumes; oxygen, 1.12; azote, 0.54; total, 5.95 volumes. The arterial blood of the calf contains more and the venous blood less oxygen than that of the horse. M. Magnus observes that these experiments, and others which we have not copied, appear to shew that the gases contained in the blood of the animals amount to about one-eighth or one-tenth of the quantity employed. He admits, however, that the experiments are not absolutely precise, because they were not all continued the same length of time, &c. But he observes,

that, as the proportions between the oxygen and carbonic acid are invariably the same, these results may be regarded as satisfactory. With regard to the theory of respiration, all experimentalists agree as to the reciprocal proportions between the carbonic acid expired and the oxygen absorbed; while, however, some of them are of opinion that those quantities are always equal, as must happen if the oxygen gas were employed merely in the formation of carbonic acid in the lungs. There are chemists whose results shew that more oxygen is inspired than carbonic acid expired. Messrs Allen and Pepys observed that this was constantly the case where the same air was repeatedly respired. M. Magnus adds, that this fact, so inexplicable by other theories, is an immediate consequence of the hypothesis founded on the law, that a liquid holding a gas in solution parts with it when it comes in contact with another gas. Another circumstance noticed by Messrs Allen and Pepys, is as inexplicable as the preceding, namely, that by the respiration of oxygen, or of a mixture of oxygen and hydrogen, azotic gas is constantly expired, the volume of which is proportional to the bulk of the animal; this proves that it cannot at all be attributed to the air. It now remains to be shewn that the carbonic acid extracted from the blood is in sufficient quantity to account for that which the lungs expire. The results obtained on this subject are discordant; those of Messrs Allen and Pepys evidently exceed what they should be; for Berzelius has shewn that if correct, it would require six pounds and a quarter of solid nourishment in twenty-four hours, to produce the quantity of carbon consumed. Taking, then, the results by Davy as a mean of those of Lavoisier, and Allen and Pepys, although perhaps a little too high, we shall have thirteen cubic inches as the quantity of carbonic acid gas expired by a man. If it be further admitted, that at each pulsation of the heart an ounce of blood arrives at the lungs, seventy-five pulsations in a minute would convey five pounds of blood in the same time. This is the minimum quantity which can be admitted; for it is very probable that five pounds of blood pass through these organs every minute; these five pounds produce thirteen cubic inches. It has been already mentioned that the blood contains at least one-fifth of its volume of carbonic acid; and as a pound is equal to twenty-five cubic inches, each pound of blood would contain at least five cubic inches of carbonic acid. It will be observed that no circumstance opposes the proposed theory; hence the experiments prove that the quantity of carbonic acid contained in venous blood, is more than sufficient to furnish the quantity required.

ZOOLOGY.

2. *Presentation of the Wollaston Medal to Professor Owen.*—At a late meeting of the Geological Society of London, the President announced that the Wollaston Medal for the last year had been awarded to Professor Richard Owen, and on presenting it to him Mr Whewell expressed himself in the following terms:—

"Mr Owen,—I have peculiar pleasure in presenting you with this medal, awarded to you by this Society, for your services to fossil zoology in general, and in particular for the description of the fossil mammalia collected by Mr Darwin. I trust it will be a satisfaction to you to receive this our testimony of the success with which you have cultivated that great science of comparative zoology, to which you have devoted your powers. I trust it will add to your satisfaction, to consider, that the subject which we more peculiarly wish to mark on this occasion,—the study of fossil zoology, is one to which the resources of your science were applied, while the subject was yet new, by that great man—John Hunter,—whose museum and whose reputation are so worthily assigned to your care. I trust also that this medal, thus awarded to you, at the outset, if I may so say, of an enlarged series of investigations, will convey to you the assurance that, in your progress in such researches, you carry with you our strong interest in your endeavours, and our high esteem of your powers and your objects; and will convince you, that in all your successes, you may reckon upon our most cordial sympathy in the pleasure which your discoveries give."

3. *Communication of the Poison of Hydrophobia or Rabies from one human being to another.* By Dr HAXTHAUSEN, District Physician at Neisse.—A servant received a slight wound from a young shepherd's dog in the right hand, and eight weeks afterwards, became affected with hydrophobia, although he

placed the most implicit confidence in a quack remedy employed by the peasants in his neighbourhood, which was applied immediately after the accident. He had felt sharp pain proceeding from the cicatrix of the wound up the arm to his breast, many days before the appearance of the disease. No vesicles appeared on the tongue, but there was swelling of the glands, and although he was in full possession of his senses, yet he had a strange unaccountable feeling at intervals. The longer those periods lasted, so much the longer and more stormy were the hydrophobic attacks. In one of these paroxysms, rendered remarkable by his spitting, biting, and scratching, he suddenly broke away from his attendants, and scratched the face of a servant girl who was present. This girl, a strong-minded phlegmatic person, about twenty years of age, immediately washed her face, and regarding the accident as an indifferent occurrence, thought no more about it, until the night of the 16th May, when she found herself indisposed. Her temperament had become hasty and exalted, and quite different from what it had been formerly. Her eyes became bright, and at times sharply fixed; her thoughts, contrary to her usual wont, seemed busy; sometimes she would appear to listen, then feel astonishment. These symptoms led to apprehensions of a serious illness, but before this had occurred, the girl was brought home to her parents in a neighbouring province of Austria. From thence we received information that hydrophobia had set in with all its symptoms. She felt shuddering and spasmodic constriction of the throat at the sight of fluids, with sensation of choking and twitching motions of the whole body. The attacks came on periodically, and were particularly characterized by wild shrieking, and a wish to bite and spit, as well as to destroy every thing. The latest news we received was that the girl was still alive, and that the fits, with even increasing periods of intermission, had become gradually slighter, and assumed in a remarkable manner the nature of epileptic convulsions, although previously she never had had an attack of this kind. These paroxysms occurred very seldom in the latter periods and were slight, yet they were principally excited by some remembrance of the man who had died. The medical treatment, after the abstraction of blood, had been of a simple nature.—*Dublin Med. Journ.* vol xiii. p. 142.

GEOLOGY.

4. *On the Frozen Soil of Siberia*, by Professor Baer of St Petersburgh; communicated to the Geographical Society, through Vice-Admiral Von Krusenstern.—It has long since been ascertained, says M. Baer, that over a great extent of country, the soil in Siberia is never entirely free from ice; during the summer, the surface of the ground is, to a greater or less depth, thawed; but at some distance from the surface, a bottom of perpetual ice is met with. Gmelin the elder, in his travels in Siberia, states, that shortly after the foundation of the town Yakutzk* (in Lat. 62½° North, Long. 130° East nearly), at the end of the seventeenth century, the soil at that place was found to be frozen at a depth of 91 feet, and that the people were compelled to give up the design of sinking a well. Many other facts of this description were collected by travellers about the middle of the last century, but these facts seem not to have been generally credited; and even in 1825, Leopold von Buch, a philosopher whose opinion is of the greatest weight in all questions connected with the physical condition of the globe, rejected these statements as entirely erroneous; yet they have been corroborated in our days by the travels of Erman and Humboldt. Until very lately, nothing was known respecting the thickness of the frozen surface; but within these few years a merchant of the name Schargin, having attempted to sink a well at Yakutzk, was about to abandon the project in despair of obtaining water, when Admiral Wrangel persuaded him to continue his operations till he had perforated the whole stratum of ice. This he did, and kept a complete journal of his work. The well or pit of M. Schargin had been sunk to the depth of 382 feet, and at that distance from the surface, the soil was very loose, and the temperature of the earth ½° Reaumur (31° Fahr.), but nearer the surface it had been much lower, and had increased as follows: Reaumur, 6° at some feet below the surface; 5° at 77 feet; 4° at 119 feet; 2 at 217 feet; 1½° at 305 feet; ½° at 350 feet; ½° at 382 feet. As the soil had already become loose at 350 feet, and as the aperture of the well was eight feet square, and the work carried

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* For remarks on this subject by Professor Bischof, see this Journal, vol. xxiii. p. 238.

on partly during winter, when, of course, the column of cold air must have rushed into the pit and chilled the temperature, it is probable that the spot at which the thermometer marked the frozen point, was at the depth of 350 feet. This immense thickness of ground ice would prove that Siberia must have been for a long period in the same physical condition as it is at present. In the actual state of our information on this subject, it is impossible to determine how widely this layer of ground-ice is spread under the surface of Siberia; yet, we know enough to say that it extends over an immense tract of country. Humboldt found the soil frozen at a depth of 6 feet at Bosgolowsk, near the Ural, in 60° North Lat. Near Beresow, Erman found the temperature of the earth, at a depth of 23 feet, still + 1°.6, (35½° F.), but in 1821 a dead body was disinterred, which had been buried 92 years before; the earth around it was frozen, and the body did not shew any signs of decomposition. It has long been known that at Obdorsk, in North Lat. 68°, the ground is always frozen. Near Tobolsk no ice is found in the soil, but as we proceed to the eastward, the ground ice advances farther north. It is to be hoped that measurements of the temperature will shortly be made at different depths at Yakutzk, and by methods which M. Schargin was unable to employ; also it is desirable to institute an inquiry as to the depth at which the ice annually disappears near the surface, and collect information on the depth of ground ice generally in Siberia. It would also be highly gratifying to me, and extremely interesting to science in general, if the Geographical Society of London would collect information respecting the extent of the layer of ground ice in North America, the thickness which it attains, and how much of it disappears by the summer heat, in those countries over which the factories of the Hudson's Bay Company are disseminated. At the termination of the reading of this paper, an animated discussion took place on the frozen soil of Siberia, in which the members stated their views on the subject. It appears to be generally considered, that the experiment at Yakutzk had not been made with sufficient care, to authorize the belief that the frost penetrates to so great a depth as 350 feet below the surface of the globe; also that the statements of M. Arago and Von Buch, and others in our own country, on the increase of temperature in proportion to the distance from the surface, were fully borne out by the observation of M. Schargin, and almost exactly in the same ratio as hitherto found. Captain Back stated, that in his many years' experience in the cold regions of North America, even in the height of an Arctic summer, he had never found the ground thawed more than four feet below the surface; but that experiments on the subject were very much to be desired.

5. *Mummy in a Peat-Moss.*—Among the curiosities recently added to the Museum of the Royal Society of Northern Antiquities at Copenhagen. there is one of a singular nature and great historical interest. It is the mummy of a female, found in a peat-bog near Haraldskioer, in Jutland, completely sunk in the soft ground, and fastened to a stake by means of clamps and hooks. The fragments of clothing that remained on the mummy enable the skilful antiquarians of the north to conclude, with tolerable certainty, that it belongs to the last period of paganism; and M. Peterson has endeavoured, in an able historical essay, to prove that the mummy is the body of Gunnhilda, Queen of Norway, whom King Harald Blaatand enticed, by promise of marriage, to come to Denmark in 965, when he put her to death by sinking her in a bog.—*Athenæum*, No. 540, p. 168.

6. *Further Discovery of Fossil Remains of Quadrumana in India.*—In a former volume of this Journal, we gave an account of the discovery of fossil remains of a quadrumanous animal in France; afterwards of the finding of similar remains in India, which, we have now to remark, were discovered long before it was known that similar remains occurred in the strata of Europe. We have hitherto received no particulars as to the more recent discoveries in India, further than that three new fossil species have been dug out of the Sivalic tertiary strata.

7. *The Waterfall of Glomach, in the Parish of Kintail, in the County of Ross and Cromarty.*—In this parish there is one of the highest and finest waterfalls in the kingdom, well known in the district by its appropriate name, Glomach. It is situated in a remote and uninhabited valley, on the estate of Mr M'Kenzie of Applecross, and about seven miles from the inn of Shealhouse. It merits a special notice in a national work of this kind, and will most amply reward any fatigue the tra-

veller may undergo in approaching it from the plains. Its height, lately ascertained (without instruments) as accurately as the nature of the ground admitted of, is 380 feet. At the distance of about fifty feet from the bottom, the water meets with a slight interruption from a shelving projection in the rock. This, however, adds to, rather than subtracts from, the peculiar interest and grandeur of the scene,—forming a kind of resting-place for the eye in surveying this stupendous fall, and giving occasion to an increased volume of spray, which must ever contribute to the imposing appearance of such scenes. There is, however, independent of this break, at any season, and in any weather, a mighty fall of 300 feet. But the interruption alluded to is not at all perceptible when there is any great body of water in the river (Girsac); and, on such occasions, the fall is unbroken, terrific and sublime. The best view is obtained from a solitary tree, about 100 feet down the ravine, to the south-west of the fall, in a situation the most favourable possible for getting a complete view of the whole scene; for here, a narrow neck of rock, covered with long heath, stretches forward towards the water, enabling the visitor to occupy a station in the very centre of the tremendous objects by which he is surrounded. As there is no path, the tourist will at once perceive the necessity of using much caution in descending to this spot. On looking around him, he will find the range of his vision fearfully limited, by objects vast and immense, concentrated in fearful magnificence before him, and in almost alarming proximity to him. He is apt, however, to consider his situation more dangerous than it really is. From the tree, the scene is extremely grand. The water appears to issue from an oblong fissure in the rock, from whence, with a fearful rush, it dashes its way, chiefly in one great column, to the pool below, from which, at this station, no outlet can be perceived. The immediate neighbourhood of the fall is amazingly wild and barren. In this respect, it differs essentially from its neighbour of Foyers. Here and there may be seen a tuft of grass, or probably of breckens, or a mountain saxifrage, clinging to the great naked sheets of perpendicular rocks, and adding, if possible, to the general grandeur of the scene.—*Statistical Account of Scotland*, No. xii. p. 173.

8. *On two examples of Transported Blocks, on the South*

Coast of Finland, by K. E. Von Baer.—I have questioned the naval officers employed on the survey of the coast of Finland, as to the opinions which prevail there regarding the alterations of the level of the sea. Hitherto the expedition has been employed only on the south coast, as the extraordinary number of bays and small islands retards greatly its progress. On this south coast there is at least no general opinion among the fishermen respecting the sinking of the level of the sea, or the elevation of blocks of stone and rocky points. The older collected accounts also of this phenomenon on the coast of Finland relate only to the coast as far down as the neighbourhood Abo; but it is said that, on the south coast of Finland, the bottom of the sea and the position of the blocks are extremely changeable, so that, at places where a number of large blocks lie together, these sometimes suddenly disappear, and, perhaps, coarse sand or some other bottom is found at a much less considerable depth, inasmuch as a new deposit has been formed on the blocks. Sometimes, on the other hand, such a deposit has been carried away. These changes have been ascribed partly to the power of moving water, and partly to that of ice. The ice, on these extremely indented coasts, not only attains a considerable thickness, partly owing to the small quantity of salt contained in the water, and partly to the enclosed form of the Gulf of Finland; but also, being broken up by storms, it becomes heaped up in the same manner as has been observed, and so graphically described by Wrangell as occurring in the Frozen Ocean. It is a necessary consequence that blocks should be transported by these masses of ice. Two examples of such removals of masses of granite, which the pilot Ziwolka has communicated to me from his journal, are among the most remarkable of those respecting which we have authentic information. Hence, I think it proper to lay them before the Academy, and so much the more, because both cases have occurred rather recently; and if there should be any error or incorrectness in the account, it would be easy to obtain fuller information. At all events, more detailed particulars and drawings would be desirable. One of the instances is particularly remarkable, from the height to which the travelled stone has been carried. Near Kittleholm, in the vicinity of Sweaberg, there is to be observed, on a fixed rock which has the name of *Witheller,* a loose reposing stone of considerable size, which, seen at a distance, is so strikingly like a seal, that the sailors of the expedition gave it that name. The height of its position is about three fathoms above the level of the sea. The inhabitants of the coast assert that the stone now described first made its appearance in the year 1814 or 1815. The second instance is especially curious from this circumstance, that the inhabitants of the coast declare that they are able to recognise the stone perfectly, and that thus they can prove a journey of 250 fathoms, or half a verst, in one winter. The block in question reposes at present on another larger one. Its transport took place about the year 1806 or 1807; and it is also in the neighbourhood of Kittelholm that it occurs, but on the opposite side from the first-mentioned block. These notices are not unimportant for the theory of the distribution of the granite blocks of the north, although they are just as insufficient to explain the phenomena as a whole, as are all the other known examples of the wanderings of stones that have taken place in historical times. They are also of importance in lending strength to the conviction, that only marks cut in the fixed rock can afford sure data respecting the changes of the level of the sea, in relation to the surface of the land.*

9. *Raised Beaches at Coquimbo.*—In a letter lately received from Mr Pentland, H. M. Consul-General in Bolivia, and dated Tacna, Peru, September 3. 1837, he says: "I had occasion to observe at Coquimbo, raised beaches on a very large scale, and attaining an elevation of 400 to 500 feet above the present sea-level. They consist of beds of sea-sand, alternating with others, exclusively formed of large oysters, and in general capped by a mass of boulders and gravel, some of the former weighing several tons, and covered with parasitic marine mollusca. It is this modern marine deposit which forms the parallel roads spoken of by Captain Hall, and referred to by Lyell, and has evidently been raised at a very modern

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* An interesting account of a travelled stone, by Sir Thomas Dick Lauder, Bart., is contained in the Memoirs of the Wernerian Natural History Society, vol. iii. p. 251.

period, many of the shells preserving their brilliant colours. The vicinity of Coquimbo is composed of a transition granite, with masses of porphyry in veins, and both contain the rich metallic veins of that celebrated metalliferous district. The Andes in this neighbourhood are very high, and if the position of the peak seen from the port is accurately laid down on the maps (and which I had no time to verify), its elevation must exceed 20,000 feet, according to some zenith distances I took of it. The country around the town from which I now write is an arid sandy desert, without a trace of vegetation. It is covered with loose sand of the new red sandstone series, through which the quartziferous trachyte rises near Tacna, and continues to form a band at the base of the Andes."

10. *Philipi on the Subfossil Shells of Pouzzuoli and Ischia.*—Professor Philipi of Berlin, has lately published a memoir on the subfossil marine shells of Pouzzuoli and of the island of Ischia. At Pouzzuoli the shells were collected in an excavation lately made for the foundation of a new hospital, and all belonged to species living at present in the Mediterranean, with the exception of the *Diplodonta dilatata*, which is found in the Red Sea, but which has probably been met with in the Mediterranean, and been cited by Payrandeau under the name of *Lucina lactea*. M. Philipi deduces from the occurrence of these shells at Pouzzuoli, a proof that the land has been submerged since the commencement of the present epoch; and indeed this phenomenon seems connected with that of the temple of Serapis. In the Island of Ischia the author collected ninety-two species of shells, of which three, viz. *Terebratula bipartita*, *Hyalæa depressa*, and *Tornatella elongata*, have not yet been found in the Mediterranean. The bed which contains the shells is a clay employed by the potters, and is the product of the decomposition of volcanic ashes; it frequently includes half decomposed fragments of pumice. M. Philipi draws the conclusion that the *soulèvement* of the Island of Ischia has been much posterior, not only to the formation of the subapennine hills, but also to thatof the tertiary formation of Sicily, in which a fifth at least of the fossils belong to extinct species. According to the author this *soulèvement* must have been contemporaneous with the diluvian period.

NEW PUBLICATIONS.

1. *The Zoology of the Voyage of H. M. S. Beagle, under the command of Captain Fitzroy, during the years* 1832 *to* 1836. *Part* 1. *Fossil Mammalia.* 4to, with numerous plates: Smith, Elder and Company, London. By Professor Owen.

This magnificent and most interesting work, now publishing under the patronage of Government, is to appear in parts. The first part, that now before us, contains a geological introduction by Mr Charles Darwin, who accompanied Captain Fitzroy, explanatory of the nature of the alluvial and tertiary deposits in South America, in which the fossil mammalia are found; but the chief part of this number is written by Professor Owen, considered as the highest authority in comparative anatomy in this country. He says, " It is remarkable that all the fossils collected by Mr Darwin belong to herbivorous species of mammalia, generally of a large size. The greater part are referable to the order which Cuvier has called Edentata, and belong to that subdivision of the order (*Dasypodidæ*) which is characterised by having perfect and sometimes complex molar teeth, and an external osseous and tessulated coat of mail. The megatherium is the giant of this tribe, which at the present day is exclusively represented by South American species, the largest (*Dasypus Gigas*, *Cuv.*) not exceeding the size of a hog. The hiatus between the living species and the megatherium is filled up by a series of armadillo-like animals, indicated more or less satisfactorily by Mr Darwin's fossils, some of which species were as large as an ox, others about the size of the American Tapir. The rest of the collection belongs, with exception of some small Rodents, to the extensive and heterogeneous order Pachydermata; it includes the remains of a mastodon, of a horse, and of two large and singular aberrant forms, one of which connects the Pachydermatous with the Ruminant order; the other, with which the descriptions in the following pages commence, manifests a close affinity to the Rodent order."

The first fossil animal mentioned by Professor Owen is named *Toxodon Platensis*, which he describes as a gigantic extinct mammiferous animal, referable to the order Pachydermata, but with affinities to the Rodentia, Edentata, and herbivorous Cetacea. From the dimensions of the cranium, it would appear that the Toxodon Platensis must have been as large as the colossal megatherium. The osteological details are given in a most interesting manner; but we regret that our limits prevent us entering into details. The next extinct fossil animal described is named *Macrauchenia Patagonica*, which is a mammiferous quadruped, referable to the order Pachydermata, but with affinities to the Ruminantia, and especially to the Camelidæ. This is a very beautiful piece of investigation, and proves the singular address and skill of our author,—for, furnished only with a few bones of the trunk and extremities, without a fragment of tooth or of cranium, to serve as a guide to the animal's position in the zoological scale, he has been able to refer it to its place in the system.

2. *Malacologia Monensis;* by Edward Forbes, Esq., President of the Royal Physical Society of Edinburgh. Carfrae & Son, Edinburgh. Pp. 63, with plates.

This interesting natural history of the mollusca of the Isle of Man and the neighbouring sea, is the first publication of an accomplished and intelligent young gentleman, whose extensive scientific knowledge and acquirements entitle us to look forward with pleasure to his promised account of the higher animals, and also the plants, of his native isle.

3. *Essay on the Antiquity of the Hindoo Medicine;* by Professor Royle of King's College, London. 8vo, pp. 196. W. H. Allen & Co., Leadenhall Street, London.

This very amusing and curious volume, has been already so fully noticed in the different literary and medical journals, that we have only the agreeable duty of recommending it to the notice of our general and medical readers.

List of Patents granted in Scotland from 29*th December* 1837 *to* 13*th March* 1838 *inclusive.*

1. To William Neale Clay of West Bromwich, in the county of Stafford, manufacturing chemist, for an invention of "improvements in the manufacture of iron."—29th December 1837.

2. To James Macnee, coach-maker, George Street, Edinburgh, for an

invention of "an improvement or improvements in carriages."—13th January 1838.

3. To **Jehiel Franklin Norton**, of Manchester, merchant, for an invention, communicated by a foreigner residing abroad, of "certain improvements on stoves or furnaces."—15th January 1838.

4. To **Daniel Stafford**, of New North Street, Red Lion Square, in the county of Middlesex, gentleman, for an invention of "improvements in carriages."—15th January 1838.

5. To **William Losh**, of Bentonhall, in the county of Northumberland, Esq., for an invention of "improvements in decomposing muriate of soda (common salt), part of which improvements are also applicable to the condensing vapours of other processes."—26th January 1838.

6. To **Thomas Moor**, of Ison Green, in the county of Nottingham, lace-manufacturer, for an invention of "improvements in machinery for frame-work-knitting."—29th January 1838.

7. To **Luke Barton**, of Arnold, in the county of Nottingham, hosier, for an invention of "certain improvements in machinery for frame-work knitting."—31st January 1838.

8. To **Ambroise Ador**, of Leicester Square, in the county of Middlesex, chemist, for an invention of "certain improvements in producing or obtaining motive power."—8th February 1838.

9. To **Henry Davies**, of Stoke Prior, in the county of Worcester, engineer, for an invention of "certain improved apparatus or machinery for obtaining mechanical power, also for raising or impelling fluids, and for ascertaining the measure of fluids."—9th February 1838.

10. To **David Wilkinson Sharp**, of Bingley, in the county of York, worsted-spinner, for an invention of "certain improvements in machinery or apparatus for warping worsted, linen, cotton, silk, or woollen yarns."—9th February 1838.

11. To **James Matley**, of the city of Paris, in the kingdom of France, and of Manchester, in the county of Lancaster, gentleman, for an invention, communicated by a foreigner residing abroad, of "certain improvements in machinery for the operation of tiering used in the printing of cotton, linen, and woollen cloths, silks, papers, and other substances, in which block-printing is or may be applied."—13th February 1838.

12. To **Ambroise Ador**, of Leicester Square, in the county of Middlesex, for an invention of "certain improvements on lamps or apparatus for producing or affording light."—19th February 1838.

13. To **William Palmer**, of Sutton Street, Clerkenwell, in the county of Middlesex, manufacturer, for an invention of "improvements in printing paper-hangings."—19th February 1838.

14. To Sir **James Caleb Anderson**, of Buttevaut Castle, in the county of Cork, Baronet, for an invention of "certain improvements in locomotive engines, which are partly applicable to other purposes."—3d March 1838.

15. To **Morton William Lawrence**, of Leman Street, Goodman's Fields, in the county of Middlesex, sugar-refiner, for an invention of "certain improvements in the process of concentrating certain vegetable juices, and saccharine solutions."—6th March 1838.

16. To **John Clark**, the younger, of Mile End, Glasgow, cotton-spinner, for an invention of "improved machinery for turning, some parts of which may be applicable to other useful purposes."—6th March 1838.

17. To **John Edwards**, of Lincoln's Inn Fields, in the county of Middlesex, pen-manufacturer, for an invention of "certain improvements in instruments used in writing."—8th March 1838.

18. To **Julian Augustus Tarner**, of Henry Street, Liverpool, in the county of Lancaster, architect, for an invention of "an improved method of propelling vessels through water."—13th March 1838.

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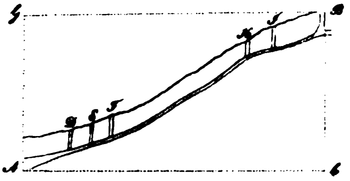

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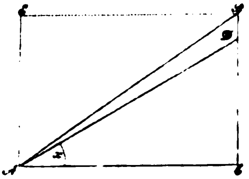

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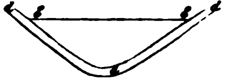

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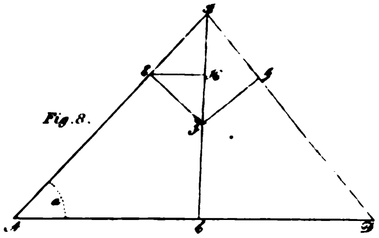

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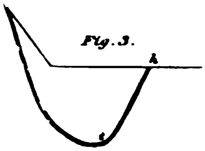

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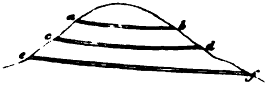

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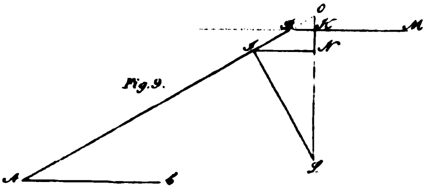

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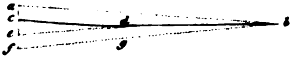

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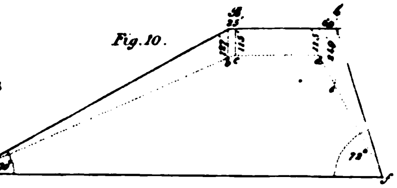

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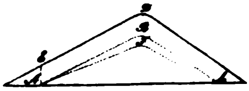

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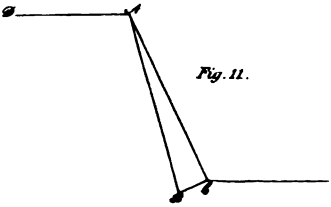

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